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HOW TO DETERMINE THE GRADE OF EXPANSION AND THE SIZE OF A STEAM ENGINE WHICH IS TO PERFORM A GIVEN DUTY WITH THE LEAST TOTAL EXPENDITURE OF MONEY PER WORKING HOUR.

By L. d'Auria.

Denote by

P, the given duty of the engine in horse-powers;

N, number of revolutions per minute;

p_1, absolute initial pressure;

p_2, back pressure, including friction of engine;

V, volume of steam cylinder (unknown);

x, grade of expansion (unknown).

When the ordinary law of expansion is admitted, the two variable quantities V and x are connected by the equation

$$V = \frac{33000 P x}{N\{p_1(1 + \text{hyp. log.}\,x) - p_2 x\}}.$$

Since P is known, it would seem rather too subtle to establish any relation between the wages to be paid to fireman and engineer, and the variable quantities x or V; at any rate, would be found impracticable. Hence, those items of expense must be entered as constant charges of cost of power. Moreover, if by means of the formula,

$$\text{HP} = \frac{NV\,\{p_1(1 + \text{hyp. log } x) - p_2x\}}{33000x},$$

all other things being given, we determine several values of V corresponding to different grades of expansion x; and afterwards with such data, and the ordinary tables of cylinder condensation, we compute the quantity of steam required in each case, the variations in the latter quantity will be found rather small in comparison with the variations of x and V. Now, since in purchasing a boiler, a liberal allowance is generally made on the steaming capacity strictly necessary to run the engine, it can be seen that from a practical point of view it is not necessary to establish any relation between the capacity of the boiler and the grade of expansion of the engine. It is sufficient to know the duty of the engine in order to fix the capacity of the boiler, upon an assumed low grade of expansion; consequently, all the items of expense due to the cost of boiler, as interest, depreciation of value, and repairs, must also be entered as constant charges of cost of power. We can then represent by a constant K, the sum of wages of fireman and engineer, interest and depreciation of value on cost of boiler, repairs of boiler, all computed per working hour; although such quantity K is different for each particular case.

While a variation in the volume of the steam cylinder can hardly affect the capacity, and, therefore, the cost of boiler, it affects, in a considerable degree, the cost of the engine, which, according to creditable authorities, seems to vary in direct proportion with V. Hence, the items of expense due to the cost of engine, as interest, depreciation of value, repairs, and we may include, also, cost of oil, all computed per working hour, must be expressed by a term AV, where A is to be determined by experience.

Denoting by w the weight per cubic foot of steam of pressure p_1; the theoretical consumption of steam per hour will be

$$\frac{120\,wVN}{x}\text{ lbs.}$$

The percentage of this allowed for condensation and clearance can be expressed by mx^a; m and a, being numbers to be found experimentally. If B is the cost of fuel to make one pound of steam, the actual cost of steam per working hour will be

$$\frac{120\,wBVN(1 + mx^a)}{x};$$

and the total cost of power, will be

$$K + V\left\{A + \frac{120\,wBN(1 + mx^a)}{x}\right\}$$

Now we have

$$\text{Efficiency} = \frac{\text{HP}}{K + V\left\{\frac{A + 120wBN(1 + mx^a)}{x}\right\}};$$

and the object of our problem is to make this a maximum with the condition that only V and x are variable quantities. In such case we have

$$\text{der. } V\left\{A + \frac{120\,wBN(1 + mx^a)}{x}\right\} = o,$$

and substituting the value of V in function of x given above, we find

$$\text{der. } \frac{Ax + 120\,wBN(1 + mx^a)}{p_1(1 + \text{hyp. log. } x) - p_2 x} = o.$$

The result of this derivation furnishes the relation

$$\text{hyp. log. } x = \frac{120\,wBN(1 - \frac{p_2}{p_1}x)\,(1 + mx^a - amx^a)}{Ax + 120\,wBNmax^a}$$

which determines the most economical point of cut-off.

In order to make this formula more ready for use, let C represent the cost in dollars and cents of one ton of coal of 2,000 lbs; and n, the number of pounds of water evaporated per pound of coal per hour. We have then

$$B = \frac{C}{2{,}000\,n}$$

and substituting we find

$$\text{hyp. log. } x = \frac{wCN(1 - \frac{p_2}{p_1}x)\,(1 + mx^a - amx^a)}{16{\cdot}666\,nAx + wCNmax^a}. \qquad (1)$$

From Barrus' tables of cylinder condensation for unjacketted factory engines we have deduced $m = 0{\cdot}16$, and $a = \frac{1}{2}$, approximately; hence we can write

$$\text{hyp. log. } x = \frac{wCN(1 - \frac{p_2}{p_1}x)(1 + 0{\cdot}08x^{\frac{1}{2}})}{16{\cdot}666\, nAx + 0{\cdot}08\, wCNx^{\frac{1}{2}}} \tag{2}$$

Put $C = \$5.00$; $n = 9$ lbs; and assume for a certain style of condensing engine $N = 100$; $p_1 = 90$ lbs; $p_2 = 5$ lbs; $w = 0{\cdot}2092$ found by tables. Suppose that an engine $22'' \times 36''$ of the same style costs \$6,900, and allow 12 per cent. for interest, depreciation of value and repairs; \$0.04 per working hour for oil. We have then

$$AV = \frac{12 \times 6{,}900}{300 \times 10 \times 100} + 0{\cdot}04 = \$0{\cdot}316,$$

and

$$A = \frac{0{\cdot}316}{V} = \frac{0{\cdot}316}{7{\cdot}92} = 0{\cdot}04.$$

Substituting all the above date in equation (2) we find approximately

$$x = 5{\cdot}5$$

Assume $m = o$, in equation (1), that is, no condensation nor clearance, we have

$$\text{hyp. log. } x = \frac{wCN(1 - \frac{p_2}{p_1}x)}{16{\cdot}666nAx}$$

Substituting for w, C, N, n, p_1, p_2 and A, the above values, we find

$$x = 6{\cdot}2;$$

that is an augment of less than 13 per cent. in the grade of expansion, which shows, that considering the amount of condensation given by Barrus' tables as a maximum in practice, any discrepancy between this and the real amount of condensation which may take place in different engines cannot but little affect the most economical point of cut-off determined after equation (2). Hence this equation can be considered as practically correct in all cases, and as such should at once be adopted by engine makers for the pecuniary benefit of their customers and the dignity of the profession.

It remains now with the leading authorities to encourage such adoption by publicly expressing their approval, if the explicitness and correctness of our method warrants it, which we trust.

PRESENT STATE OF THE SUBJECT—"HEAT OF COMBUSTION OF COAL."

By Chief Engineer ISHERWOOD, U. S. Navy.

The experiments of Dulong, Favre, Silbermann, Andrews and other distinguished scientists, have determined the heats of combustion of solid carbon and sulphur, and of hydrogen, olefiant, and marsh gases, within very close limits, the experimental discrepancies being quite inconsiderable. Now the combustible portion of coal is composed of these substances, and did they exist in it in their respective solid and gaseous states, there could be no question that the heat of combustion of coal, as a whole, would be the aggregate of the heats of combustion thus determined of its constituents multiplied by their relative weights. But the hydrogen, olefiant and marsh gases exist in the coal in the solid state, and as they gasify at lower temperatures than their temperatures of ignition, they are, when the coal is subjected to heat, first gasified and afterwards burnt. Now, as some heat is evidently converted into the work of this gasification, the heat of combustion of the coal, as a whole, composed of the aggregate of the heats of combustion of its constituents determined for their respective solid and gaseous states, must be less by that much. The problem is then to ascertain what portion of the heat of combustion of the coal as above determined, is expended in gasifying its volatile constituents; the remaining portion being its proper heat of combustion, that is to say, the heat available for external uses. I am not aware that any direct attempt has ever been made in this direction to solve the problem.

That the heat required to gasify the volatile constituents of coal is small, is evident from an examination of the results of the process of manufacturing illuminating gas. For example: Ordinary dry bituminous coal when retorted for this purpose leaves about 63 per centum of its weight as dry coke, which, sponge-like, rapidly absorbs a considerable amount of moisture from the air. This coke has a much less calorific effect than an equal weight of the coal from which it was derived, owing to its greater percentage of ash, and less percentage of hydrogen, so that the 63 per centum of coke has not more than 60 per centum of the calorific power of the coal from which it was made. Of this coke about three-eighths are consumed in volatilizing the gas from the coal, which makes the gasification cost *commercially* ($\frac{3}{8}$ of

60 =) $22\frac{1}{2}$ per centum of the heat of the coal. Now the temperature of the gases of combustion, which in the furnace is about 2,000 degrees Fahrenheit, is certainly not less than 1,700 degrees after passing the retorts, leaving only 300 degrees expended in gasification, or 15 per centum of the heat in the furnace; hence, of the heat of $22\frac{1}{2}$ per centum of the coal, only 15 per centum is converted into the work of gasification, so that the latter is equivalent to ($22\frac{1}{2} \times \cdot 15 =$) $3\frac{3}{8}$ per centum of the heat of combustion of the coal. For different kinds of coal and different conditions of furnace, these figures will, of course, require corresponding modification, but they are probably nearly correct for the average of practice. Supposing them to be accurate for any given dry coal, then the heat of combustion of that coal can be calculated from the known percentage of its combustible constituents, and from their known respective heats of combustion. It will be the aggregate less $3\frac{3}{8}$ per centum of the sum of the products of these weights by these heats.

The celebrated chemists Dulong and Petit found that in the case of the combustion of cellulose, the heat obtained was only equal to what was due to the solid carbon constituent alone; the remaining constituents being hydrogen and oxygen, in the proportion in which they exist in water, it was assumed that these substances did exist as water in cellulose, the latter being chemically a hydrate of carbon, and that exactly the same quantity of heat was absorbed by their decomposition as was obtained by their recombination. On this hypothesis these chemists based their well known formula that in the combustion of dry vegetable matter (in which hydrogen is always somewhat in excess of what is required to form water with the oxygen) the heat evolved is that due to the carbon constituent and to whatever hydrogen may be in excess after saturating the oxygen constituent. This excess of hydrogen is very small in the case of wood, averaging about seven-tenths of one per centum of the wood. There is no reason for applying this formula to coal, even if it be of vegetable origin, for in that case it has been so changed chemically and physically by time, heat, pressure and its surroundings, that what may have been true of the wood cannot be properly predicted of the coal. Nevertheless, the formula of Dulong does give very close approximations to the total heat of coal, but this is probably owing to the facts:

1st. That the heat of gasification of the volatile constituents of the coal is very small; and

2d. That the weight of the hydrogen constituent of the coal required to saturate the oxygen constituent is also very small.

For example, supposing the hydrogen constituent of bituminous coal to be five per centum and the oxygen constituent eight per centum, then the latter would be saturated by one of the per centums of the hydrogen, leaving four of the per centums to be utilized for heat. Now one of the per centums of the hydrogen thus withdrawn is equal to from three to four per centum of the heat of combustion of the coal calculated from the total heats of its constitutents. But we have already seen that the heat required to gasify the volatile constituents of the coal is also from three to four per centum of the heat of combustion of the coal calculated from the heats of combustion of its constituents and their proportion by weight in the coal. The coincidence is doubtless purely accidental, and does not prove Dulong's law.

It is impossible to conceive that the heat of combustion of coal can be greater than what is due to the sum of the heats of combustion of its constituents multiplied by their proportional weights, less the heat absorbed by the gasification of the volatile constituents. After the gasification has taken place, the heats of combustion of the gases so formed can only be the same as those of the same gases formed in any other manner, and the heat of combustion of the remaining solid carbon must be the same as if it was derived in any other manner.

The subject was in this uncertain state when Messrs. Scheurer-Kestner and Meunier-Dolfus made their celebrated calorimetrical experiments at Thann, in Germany, on the heats of combustion of the Alsatian and other coals, for the Industrial Society of Mulhouse, in Alsace, Germany; and their results, together with a complete description of their apparatus and methods, were published in its well known bulletins. These experiments have a great reputation, and were made by an improved Favre and Silbermann calorimeter with all the corrections and accuracy that the researches of modern science could furnish. The calorimeter employed was tested against that of Favre and Silbermann, by ascertaining with it the heat of combustion of dry charcoal. The Scheurer-Kestner apparatus gave for that heat 8,100 calories, while the Favre apparatus gave 8,080 calories.

From these experiments the startling facts were announced that the heat of combustion of the coals not only exceeded what was due to the formula of Dulong, but that it exceeded considerably the heat of combustion obtained by multiplying the relative weights of the ele-

mentary constituents of the coal by their respective heats of combustion and adding the products together. In other words, the heat of combustion of the coal exceeded the aggregate heat of combustion of its constituents considered as solid carbon and gaseous hydrogen, and exceeded it on an average by about four per centum. If the comparison be made with the heat of combustion due to Dulong's formula, then Scheurer-Kestner's determinations exceed it on an average about nine per centum.

In the calorimetrical experiments of Favre and Silbermann on *organic* substances, the heat of combustion was always found to be less than what was due to that of the constituent carbon and hydrogen. In the single *inorganic* substance of the bisulphide of carbon, the reverse effect was found, and was left unexplained. Berthelot afterwards suggesting that the combination of the sulphur and carbon to form the bisulphide had been attended with absorption of heat, which heat was set free on the decomposition of the substance by combustion. It is remarkable in this connection that, in some calorimetrical experiments on the heat of combustion of three Russian coals, made by Scheurer-Kestner after his experiments with the Alsatian coals, the heat of combustion as found by him averaged $2\frac{2}{3}$ per centum *less* than the heat due to the constituent carbon and hydrogen taken at their full values. These experimental heats averaged almost exactly the heats due to Dulong's formula.

The results of the calorimetrical experiments made by Scheurer-Kestner and Meunier-Dolfus on the heat of combustion of the Alsatian coals, were never accepted by the British scientists, notwithstanding that no error was pointed out in either the apparatus or the method employed. Nor could the writer ever accept them although he bestowed the closest scrutiny and study upon them. He was therefore obliged to defer them until other experiments might be made that would either confirm or overthrow them. Experiments of this kind it appears have been in progress for several years in Munich, Germany, and with results which, though opposed to those of Scheurer-Kestner, are conformable to the general laws of chemistry and to common experience.

As much as is yet known of these later experiments will be found in the following note by Mr. Vinçotte, which I translate from "The Proceedings of the 7th Congress of the Engineers-in-Chief of the Asso-

ciations of the Proprietors of Steam Apparatus," held at Bordeaux, France, in 1882.

The subject is one of much importance to manufactures and commerce, and I have written this communication largely in the hope that the facts may be useful in inducing our government, in consideration of the enormous quantities of coal consumed in this country in the industrial arts, to direct that a commission of properly qualified scientists be appointed to make, together with an ultimate analysis of the coals and of their volatile parts, an exhaustive set of experiments on the heat of combustion of the different coals we produce, and on those of the more important foreign coals with which our own must commercially compete in the near future. Rightly interpreted, these heats of combustion give the relative calorific powers of the coals, and, of course, their respective commercial values.

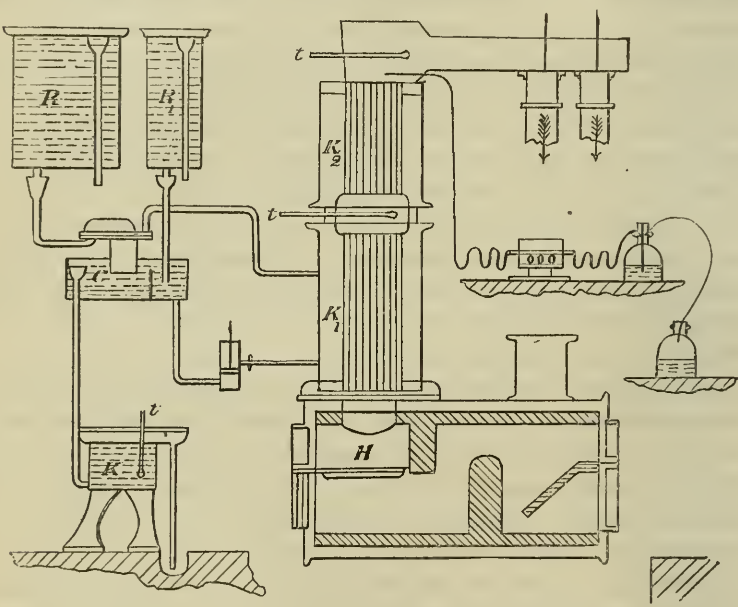
Communication presented by Mr. R. Vinçotte, Director of the Belgian Association for the Surveillance of Steam Boilers, to the 7th Congress of the Engineers-in-Chief of the Associations of the Proprietors of Steam Apparatus, held at Bordeaux, France, on the 10th, 11th, and 12th of September, 1882.

After having victoriously passed through a short period of criticism, the experiments of Messrs. Scheurer-Kestner and Meunier-Dolfus reached the position of a classic, and their results were considered absolutely certain; they were no longer even discussed. Further, they bore so striking a character of exactness that their verification was judged by other experimenters to be useless.

But to-day, on the contrary, the Experimental Station at Munich publishes results which contradict absolutely those of Messrs. Scheurer-Kestner and Meunier-Dolfus, and which I believe will interest the Congress, although I am not in possession of enough of the data of these experiments to enable me to pronounce a positive opinion on them.

The Station at Munich was established by the Bavarian government for the determination of questions relating to heating, for which purpose a boiler was constructed of such design that it could serve as a calorimeter without ceasing to be comparable as a boiler with the boilers employed in factories. It was so arranged that each portion of the heat utilized or lost could be measured separately; and that, at the same time, there could be ascertained the calorific power of a combustible and the distribution of this power in practice by a boiler.

Referring to the accompanying sketch: The furnace H is arrangeable at will for different kinds of combustibles, being adaptable for those giving a long as well as for those giving a short flame. The furnace and its masonry are entirely enclosed in two sheet-iron boxes, one within the other and separated by a water-tight space. The exterior of the outer box is prevented from radiating heat by a lagging of wood. The loss of heat due to the radiation from the masonry is



measured by means of the increased temperature of water circulating between the two sheet-iron boxes.

The hot gases of combustion, on leaving the furnace, traverse in succession two vertical tubular boilers K_1 and K_2, the latter being placed on top of the former. The exterior of both boilers is encased with Hasman's non-conducting covering.

The method of experimenting consists in first heating the apparatus until its temperature becomes stationary. Then the small quantity of ignited coal which remains in the furnace is drawn out, rapidly weighed, and thrown back, and the experiment is began with all the observations made and all the precautions taken requisite in such cases.

The heat radiated from the masonry of the furnace is measured in

the water which constantly circulates between the iron boxes enclosing it, the temperature of the water being noted at its entrance and at its exit.

The loss of heat resulting from exterior radiation, and from the conductibility of the soil on which the furnace rests, is directly measured by heating with steam taken from a locomotive the water between the boxes to a temperature a little higher than what it had during the experiment, and then ascertaining the fall of this temperature during one hour. The loss will be equal to $Q' (t' - t_1')$; Q representing the weight of water between the boxes, and $t^1 - t_1^1$ the fall of temperature.

I ought to say, in reproducing this calculation according to an article by M. Voit, that I do not believe such a process should have been adopted, because it entails a very great error since no account is taken of what the material of the boxes loses or gains at the sides of the furnace. Further, it does not appear at all easy to me to ascertain the exact value of the loss by the furnace due to radiation. The quantity of heat that the smoke abandons in each of the two boilers is measured separately and by the same method.

In the two reservoirs R and R_1 water furnished through lower orifices is kept at a constant level by an overflow, and this water, which is measured in advance, flows into a reservoir C divided into two compartments. The water in the small compartment of C is partly pumped into the boiler, and the remainder overflows into the other compartment.

The steam produced is condensed by the water entering R, and the sum of both, that is the whole quantity of water Q furnished by the two reservoirs, finally enters the calorimeter K where its temperature is measured.

The boiler's loss of heat by exterior radiation is measured directly, and *a priori*, by heating the water in the boiler by means of steam from a locomotive and then measuring the fall of pressure at the end of a given time. The heat usefully absorbed by the second boiler is measured in the same manner.

The gases of combustion are analized by a continuous aspiration across a series of absorption tubes and of a tube containing oxide of copper heated to redness.

The Munich Station commenced operations in 1879, and published in the same year a first series of seventy-six experiments made during the first eight months, which experiments, in my opinion, ought to be

considered as only preparatory for the work to be undertaken. In fact, the results of these experiments are very discordant, and show that great accidental errors are possible with the Munich apparatus. Among them are nineteen determinations of the calorific power of the same coal, so divergent that the extremes differ six per centum from the mean. It is probable that the cause of these errors was discovered, because in a second series of one hundred and sixty-six experiments the different determinations for the same coal gave differences of only one and a half to two per centum. Nevertheless, I must add that, in a vigorous criticism, M. Luders, Professor in the school of Aix-la-Chapelle, points out that in addition to the one hundred and sixty-six experiments published, the second series contained twenty-nine which were rejected without any reason being assigned; and he believes they were omitted because their results differed too greatly from the mean. Be that as it may, I shall not here discuss the value of the methods employed, because I have been able to procure only a part of the publications by the Station. I limit myself therefore to stating the results obtained, according to which the calorific power of the coals is nearly always less than what is due to the formula of Dulong and Petit. The difference is mostly very small, but, in the case of the Louisenthal coal, it rose to ten per centum.

The following table contains the values found:

Name of Coal.	Calorific power in Fahrenheit units of heat.		Name of Coal.	Calorific power in Fahrenheit units of heat.	
	Observed.	Calculat'd by the formula of Dulong.		Observed.	Calculat'd by the formula of Dulong.
St. Ingbert, I *a*..........	13867·2	14365·8	Heinitz-Dechen.........	13402·8	13647·6
" II *a*........	13077·0	13287·6	Reden-Merchweiler.	12655·8	13174·2
" III *a*......	12717·0	13123·8	Dudweiler....................	14041·8	14288·4
Mittelbexbach..........	....	....	Konig I *a*........	13114·8	13550·4
Bed No. 3......................	12803·4	12911·4	Friedrichsthal..........	12684·6	12742·2
Bed No. 6.....................	12938·4	13144·2	Ziehwald II *a*............	12110·4	12637·8
Bed No. 9.....................	12978·0	13089·6	Louisenthal................	10940·4	12119·4
Bed No. 10....................	12879·0	12729·6	Griesborn..................	10935·0	10638·0

[*Note by Translator.*—The means of the whole in the above table

shows that the observed calorific powers, or those obtained by experiment, averaged 2·352 per centum less than the calorific powers calculated according to the law of Dulong and Petit.]

The following table compares the calorific powers in Fahrenheit units of heat found by the Station at Munich, and by Messrs. Scheurer-Kestner and Meunier-Dolfus for the same qualities of coal. The figures of the latter gentlemen are for the pure coal, and in order to allow the comparison, I have placed by their side the values calculated according to the law of Dulong and Petit:

	Station at Munich.		Scheurer-Kestner and Meunier-Dolfus.	
	Observed.	Calculated according to Dulong.	Observed.	Calculated according to Dulong.
Dutweiler	14041·8	14288·4	15703·2	14349·6
Heinitz	13402·8	13647·6	15276·6	13714·2
Friedrichsthal	12684·6	12742·2	15222·6	13329·0
Louisenthal	10940·4	12119·4	14787·0	13260·6

The results obtained at the Munich Station were fiercely attacked, and they were declared false because they were in disaccord with those obtained by Messrs. Scheurer-Kestner and Meunier Dolfus. The Munich Station, before these criticisms were made, understood that their first experiments ought to be tested by comparative ones, and they recommenced the determination of the calorific powers of the same coals, requesting M. Stohman, Professor at the University of Leipzig, to make at the same time calorimetrical experiments by the ordinary method.

The results, given in the following table, vary but little, especially if there be taken into consideration the difficulty of obtaining a small sample for the calorimetrical trials, having the same composition as the mean of the coal burnt in the boiler.

M. Stohman in summing up says: You see by our figures that we have found some values in accord with the law of Dulong, and others greatly different, and these last in both directions, sometimes more and sometimes less. We have never found values as high as those of M. Scheurer-Kestner.

These figures suffice to show that if the experiments of Messrs.

Scheurer-Kestner and Meunier-Dolfus are exact, those of the Munich Station cannot be; and, in this connection, I ought to remark that the greatness of the errors necessary to be supposed astonishes me much, but in the absence of sufficient details, I cannot imagine how they could have been made.

Name of Coal.	Calorific Power of the Coal in Fahrenheit units of heat (ash inclusive).			
	Stohman.	Munich Station.	Differences.	
			Absolute.	Per cent.
Dutweiler	13852·8	13674·4	— 178·4	— 1·3
St. Ingbert	13777·2	13721·4	— 55·8	— 0·4
Mittelbexbach No. III	12956·4	12690·0	— 266·4	— 2·0
Louisenthal	12384·0	11970·0	— 414·0	— 3·3
Friedrichsthal	12879·0	12600·0	— 279·0	— 2·1
Thurn and Taxis	12600·0	12297·6	— 302·4	— 3·5
Gehenkinchen (gas making coal)	13302·0	13582·8	+ 280·8	+ 2·0
Gehenkinchen (locomotive coal)	14099·4	14475·6	+ 376·2	+ 1·5
Upper Silesia, Konigsgrube, Sattelflotz	12844·8	12648·6	— 196·2	— 1·5

It appears evident to me at first sight that the Munich apparatus should furnish results more closely concurrent than 12 per centum. Well made experiments on ordinary boilers come closer than that. It seems to me that once familiar with the method of experimenting, figures should be obtained not diverging more than one or two per centum. I have not, however, any reason for rejecting the figures of the second series of experiments: there is no case here of accidental errors. If the general result is erroneous, it must be due to a general error in common for all the experiments. In this connection we encounter in the first place, the determinations of the constants which represent the cooling by radiation from the furnace, from the boilers, and from the calorimetrical apparatus. It seems to me very difficult to determine these constants exactly by the method of cooling during a given time, for the apparatus cannot then be under the conditions of the experiment. It appears to me, therefore, that these constants are strongly subject to inexactness, but, on the other hand, the probable error made under this head is not enough to explain the figures obtained.

Remarks on the above by Mr. E. Cornut, Engineer-in-Chief of the Association of the North of France.

I have had occasion to specially examine the calorimetrical investigations of Messrs. Scheurer-Kestner and Meunier-Dolfus, and, for my part, have the greatest confidence in the results obtained by those able experimenters. I will remark that in a problem of this kind, the errors are in general of two perfectly distinct classes:

1st. A sensibly constant error due to the imperfection of the apparatus and of the methods employed.

2d. Error of observation which, by its very nature, can reach in one direction or the other, such or such an experiment.

This second cause of error does not seem to me to have been in action from a comparison of the results of the calorimetrical powers obtained by the method of Messrs. Scheurer-Kestner and Meunier-Dolfus and those obtained by the German laboratory at Munich; we perceive, in fact, that the case is one of a constant of from about ten to fifteen per centum applying to all the results.

Is there, then, an error of the first category in the experiments at Thann? I will remark that Messrs. Scheurer-Kestner and Meunier-Dolfus took the precaution to test their process by obtaining the calorific power of wood charcoal, and they found its value the same which had been previously ascertained by Messrs. Favre and Silbermann. Have the experimenters at Munich taken that precaution? and, if they have, what figure did they find for the calorific power of wood charcoal?

As our colleague, M. Vinçotte, promises us for the next year more details on this subject, I will leave it for the present; but I believe the experiments of Messrs. Scheurer-Kestner and Meunier-Dolfus to have been made with all possible guarantees for exactness, and in the most disinterested manner.

Low Temperatures.—Wroblewski has tried to measure the temperature of boiling oxygen. For that purpose he applied a system of thermo-electric measurements, which, in addition to being extremely sensitive, may be made to register all the sudden changes in the temperature of the medium. The indications of the apparatus have been compared with those of a hydrogen thermometer, between 100°C. and —130°C. (212°F. and 202°F.) The nature of the function which unites these indications is such as to permit an extrapolation, which indicates —186°C. (—302·8°F.) as a first approximation to the temperature which is produced in the liquifaction of oxygen Compressed nitrogen, at this temperature, solidifies and falls, like snow, in crystals of remarkable size.—*Les Mondes*, Jan. 6, 1884. C.

NEW YORK TO CHICAGO IN SEVENTEEN HOURS.

By W. BARNET LEVAN, Engineer.

[Read at the Stated Meeting, May 21, 1884.]

In this paper I propose to show how the distance between New York and Chicago can be covered in seventeen hours, *via* the Pennsylvania and the Pittsburgh, Fort Wayne and Chicago Railroads. The distance by this route is nine hundred and eight miles as follows:

	Miles.		Minutes.
New York City to Philadelphia, Mantua Station	88·26	in	100
Philadelphia to Harrisburg	103·07	in	114
Harrisburg to Altoona	131·6	in	144
Altoona to Pittsburg	116·7	in	128
Pittsburg to Alliance	83	in	91
Alliance to Crestline	106	in	117
Crestline to Fort Wayne	131·39	in	144
Fort Wayne to Chicago	148	in	162
Total miles	907·91		
Total time in minutes			1,000
Crossings at grade in Ohio 16			
" " Indiana 10			
" " Illinois 8			
Total crossings at grade 34			
Time lost in minutes by slowing down according to law			20
Total number of minutes consumed			1,020
Total time of run to Chicago in hours			17

I have selected the Pennsylvania Railroad, as that company controls the shortest and most direct route between the two cities mentioned, and possesses the further advantage of having its tenders fitted with a "pick-up" apparatus for supplying them, while running, with water from troughs placed between the rails.

To accomplish the distance in the time named is with this company only a question of additional safety gates, so as to keep the track clear through the large towns and cities scattered along the route.

I have divided the route up into eight sections, necessitating the use of eight locomotives. This, however, is because of the way the road is divided into Superintendents' Divisions, not from the necessity of changing on account of the locomotives, except on the Western Divi-

sion of the Pennsylvania Railroad, where two locomotives will be required in crossing the Alleghany Mountains.

At Philadelphia, in place of running into Broad Street Station, the locomotive and passenger car, with passengers, will be in waiting at Mantua, and take the place of the locomotive and car of passengers for Philadelphia only. At the other stations on the route the passengers can be changed in the time occupied in changing locomotives.

The ability of the locomotives of the Pennsylvania Railroad Company to perform this journey will be seen by the following indicator diagrams:

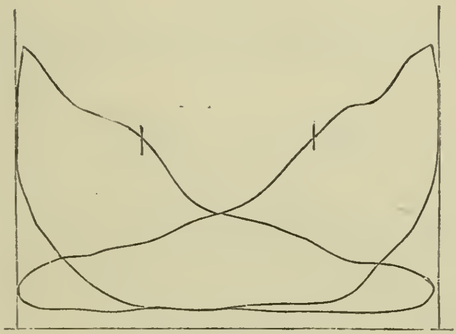


Fig. 1.

Diagram Figure 1 was taken when running at the rate of *fifty-five miles an hour*, cutting off after the piston had traveled seven inches,

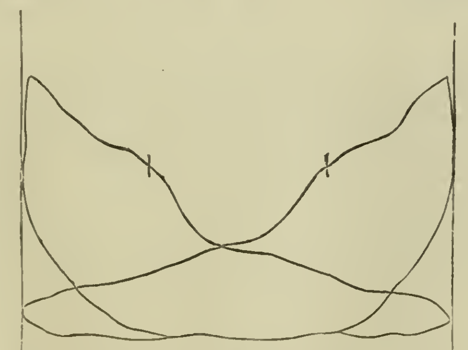


Fig. 2.

with a boiler pressure of one hundred and thirty-five pounds per square inch, an average initial pressure of one hundred and twenty-one and

one-half pounds at commencement of the stroke and eighty-four and one-half pounds at point of cut-off, and eight pounds average back-pressure.

Diagram Figure 2 was taken when running *sixty miles an hour*, cutting off at seven inches, initial pressure one hundred and nineteen and one-half, and eighty-one pounds pressure at point of cut-off, averaging eight and one-half pounds back-pressure.

Diagram Figure 3 was taken when running *sixty-four miles an hour*, boiler pressure as above one hundred and thirty-five pounds per square

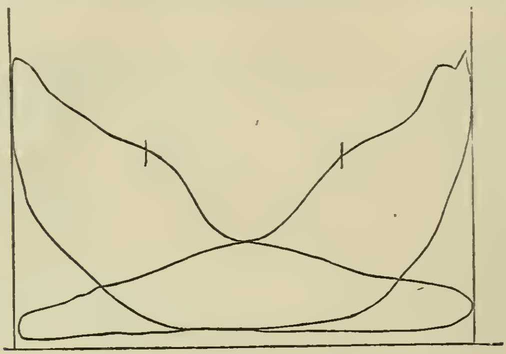


FIG. 3.

inch, initial pressure one hundred and twenty-seven and one-half, and eighty-four pounds at point of cut-off, averaging six and one-quarter pounds back-pressure.

The dimensions of these locomotives are as follows:

Diameter of cylinder in inches	18
Diameter of piston rod in inches	3
Area of piston less one-half area of piston rod in square inches	251
Length of stroke in inches	24
Diameter of drivers in inches	78
Capacity of tank in gallons	1,920
Capacity of coal box in pounds	12,000
Weight of tender loaded in pounds	56,300

Weight of locomotive in working order:

On truck in pounds	27,400
On first pair of drivers in pounds	33,600
On second pair of drivers in pounds	31,700
Total weight in pounds	92,700

The tractive force exerted for each pound of effective pressure per square inch on the piston is as follows:

$$\frac{18^2 \times 24}{78} = \frac{324 \times 24}{78} = 99{\cdot}7 \text{ pounds.}$$

The water tank, as before stated, is fitted with Ramsbottom's water-lifting apparatus for taking in a supply of water while running.

To accomplish 908 miles in seventeen hours the average miles run per hour must be *fifty-five miles.* Therefore, as the locomotive must be able to exceed the average number of miles per hour required for this purpose we will take diagram Figure 3, whose average mean pressure is 40·3 pounds per square inch and 276 revolutions per minute, averaging 677 horse-power.

$$\text{HP} = \frac{18^2 \times 0{\cdot}7854 \times 1104 \times 403}{33000} \times 2 = 677 \text{ horse-power.}$$

It is an every-day occurrence at intervals on the Pennsylvania and Bound Brook route to average for short distances *seventy miles* an hour, in fact, often a mile in *forty-five seconds,* or at the rate of *eighty miles an hour*

$$\frac{60 \times 60}{45} = 80.$$

Therefore, it is not a question of capacity of either the boiler or engines, it is simply a clear track and a disposition of the company to order it done.

It is evident from the indicator diagrams shown that the boilers are superior to the engines. The diagrams show only an average of *sixty-five per cent.* of the theoretical diagrams, while diagrams from stationary engines of similar capacity with automatic cut-off show an average of *ninety per cent.,* this difference is due to the use of the link motion in locomotive engines. The scant opening which it gives when cutting off at *six* to *eight* inches is one of its most prominent defects, as a great part of the actual boiler power is expended in forcing the steam through the narrow openings but partially uncovered by the valve, whereby a loss of over *thirty per cent.* of effective motive power is the result.

By the substitution of a separate cut-off valve similar to that adopted by Mr. A. J. Stevens, of the Central Pacific Railroad, this great loss could be overcome and there would be a great saving of fuel. This substitution would cost about $300 for each engine, and about *thirty-three per cent.* additional working power would be gained by it.

DISCUSSION.

Mr. Hugo Bilgram:—Referring to Mr. Le Van's statement that an independent cut-off will vastly increase the power, I am of opinion that if there were no wire drawing of the steam at all, only a small corner would be added to the diagram, and the power increased by probably less than 10 per cent., instead of 30 per cent. as stated in the paper. The compression would tend to fill up the clearance and prevent the loss of the steam that would otherwise be required to fill the clearance at every stroke.

Mr. Cyrus Chambers, Jr:—I am of the opinion that it is advantageous to have the excessive back pressure at the termination of the stroke, as shown by the indicator cards, and that it is not detrimental to the power of the engine, provided the compression of the exhaust does not exceed the boiler pressure, because it is utilized in the return stroke giving back to the piston all the power consumed in its compression, less friction and leakage. I agree with Mr. Bilgram on this point.

By closing the exhaust valve just in time to compress the exhaust steam at the end of the stroke up to boiler pressure, the momentum of the moving parts is overcome, all lost motion in the connections are quietly taken up at the point of stroke where the pressures from the induced and exhaust steam are equal on either side of the piston.

This makes a smooth running engine even with considerable lost motion in the connections, and as no steam is taken from the boiler for clearances, but that now in the clearances giving off its power by expansion, I see no loss except from friction and leakage.

My firm has built quite a number of engines compressing the exhaust at the end of stroke to boiler pressure with very satisfactory results.

I do not wish to be understood that we "throttle the exhaust," on the contrary we give a free exhaust until there is just enough exhaust steam left to fill the ports and clearances when compressed to boiler pressure.

We are now constructing an engine in which the terminal pressure of the exhaust (if exhaust it be) is greater than the boiler pressure, and economical results are expected.

Mr. Hector Orr made an inquiry concerning the safety of such fast running trains as the paper of Mr. Le Van described.

Mr. J. W. Nystrom:—The clearance in locomotive engines is about

that in well constructed stationary engines of the same size, but that is no good reason why the exhaust should be compressed as much as shown on these diagrams. The fact is that when the steam is cut off at an early part of the stroke by link motion, the excessive compression cannot be avoided.

The Porter-Allen engines make very good indicator cards, when running as fast as those on locomotives, showing a proper regulation of steam to and from the cylinder, and the same could be done on locomotives.

I believe Mr. Le Van is right in advocating separate cut-off on locomotives, the accomplishment of which would save at least 20 per cent. in the economy of steam. It has been stated that the economy in excessive compression is due to the saving of clearance steam. The locomotive engines have about 8 per cent. clearance, and when the steam is cut off say at $\frac{1}{4}$ of the stroke, then $\frac{3}{4}$ of the clearance steam is expanded into the cylinder, and the loss is thus two per cent. while the loss of power by compression amounts to 10 or 20 per cent.

In answer to Mr. Orr's question of safety on fast-running trains, I may say that the greatest danger is to run fast on sharp curves, which danger is said to be diminished by a peculiar railroad curve introduced in France by M. Froudé, consisting of a parabola of the third order, and called the *elastic curve.* Engineer C. A. Sundstrom, a member of the Institute, has made the elastic curve a special study, and, I believe, has laid out several of them on the Pennsylvania Railroad. I suggested to Mr. Sundstrom to read a paper on the subject at the Institute on the elastic curve. Mr. Sundstrom says that all the curves on the government railroads in Sweden are elastic curves.

Mr. Bilgram :—In reply to Mr. Nystrom's statement that compression absorbs power, let me say that in expanding engines the greater part of that power would be restored by the consequent re-expansion of the compressed steam. The short and steep compress curve of the diagrams of some engines are due to the very small clearance of such engines, a result which is not attainable on locomotives.

Mr. Le Van :—Messrs. Bilgram and Chambers forget that with link motion, when cutting off at less than half stroke, the initial cylinder pressure does not reach that of the boiler pressure by twenty-five per cent., and only momentarily at that (see Fig. 3), whereas, with an independent cut-off valve, the loss in a well-constructed engine will not be over three per cent. up to point of cut-off.

Take the left hand indicator diagram, Fig. 3, on which I have erected a theoretical diagram (see Fig. 4) from the point of exhaust opening, and represented by the dark shaded portion of the card; this is what would be produced by a perfect engine. Now, in the best practice, with stationary engines of a similar size with our fast locomotives in general use, and running at the same number of revolutions, there are stationary engines that develop *ninety-five per cent.* of the theoretical diagram, whereas diagram, Fig. 3, realizes only *sixty-five per cent.*, a loss of *thirty per cent.*

Now, I maintain that locomotive engine builders can produce just

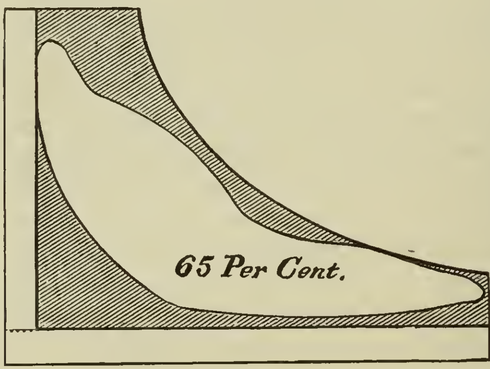


Fig. 4.

as good engines and results as stationary engine builders, and it is incumbent on them to do so.

We all know the difficulties that Mr. Corliss had to contend with when he first introduced his independent cut-off valve; it seemed incredible to those to whom he offered to sell his engine, from the fact they could not understand it, although he explained to them that the efficiency of his engine was due to a higher initial steam pressure in the cylinder, the steam line maintained without expansion, the rapid closing of the steam-valves so that all wire-drawing was prevented and the whole expansion of the steam secured; a low terminal and a free exhaust.

I do not propose to dispense with the link motion, but to add a variable cut-off valve in addition. To many this would seem an unnecessary complication, inasmuch as a separate cut-off valve would be supposed to accomplish quite as much without a link as with it.

It is well known that an engine may be run with an admission of steam to a shorter length of the stroke—in other words, with an earlier cut-off—when an independent variable expansion valve is used, than with the link alone. This being admitted, the question again comes up, "What is the advantage of the link used in addition to the cut off?"

Its advantage is simply as follows:

First.—The link is the simplest and readiest means of reversing.

Second.—While the cut-off is being run at, say, one-fourth or three-eighth stroke, the link may be worked to *vary the exhaust.* It is found to be less advantageous to hold on to the steam as long, when cutting off short, as when following for a greater length of stroke. Let an engine constructed with both a link motion and a separate cut-off valve, have the latter set at one-quarter stroke, the engine meanwhile running along at a corresponding speed; the link, which is supposed to be working the main valve at full throw, may now be pulled up, notch by notch, and it will be found that with each rise of the link and consequent shortening of the throw of the main valve, whereby the exhaust is released earlier and earlier, the engine will quicken its speed.

In the early history of the locomotive the independent cut-off valve as built from designs of Mr. Ethan Rogers, of the Cuyahoga Works, Cleveland, Ohio, and were found to be very efficient. Unfortunately at that time, these improvements were in advance of their time, on account of their additional expense, and the small amount of capital then at command of the railroad companies.

The advantages of independent variable cut-off valves over the ordinary valve controlled by the shifting link motion is well known by the locomotive builders of to-day, but they build a standard locomotive, and can ordinarily sell all they can produce at a fair price, and as long as they have customers for their make they will not change their plan of engines.

Mr. Chambers' statement as to the advantage of compression for overcoming the "jars" or "thumps" due to lost motion of the connected parts is correct, but the amount of compression for this purpose is very much exceeded in indicator diagrams Figs. 1, 2 and 3, thereby causing too great a loss of power.

To Mr. Orr's question of safety, I would say, that in England, where there are one hundred trains running daily, averaging over *fifty miles an hour,* the number of killed is only *one* in 62,000,000 of pas-

sengers carried, and with the improvements now adopted by our railway companies in constructing elastic curves, as stated by Mr. Nystrom, in the place of rigid ones, steel bridges and well-ballasted road-beds, all risks are reduced to a minimum.

ELECTRO-DYNAMICS.

By John W. Nystrom.

[Read before the Electrical Section of the Franklin Institue, June 13th, 1884.]

The object of this paper is to criticise the electro-dynamics advanced by Count Du Moncel in his work on "Electricity as a Motive Power," page 297, English edition, and to show the confusion in which that subject is yet involved.

The Count Du Moncel gives three formulas on page 299, intended to express the quantity of work accomplished by an electric current, namely as follows:

$$K = RI^2 \qquad 1.$$

$$K = \frac{E^2}{R}. \qquad 2.$$

$$K = EI. \qquad 3.$$

$K =$ work expressed in kilogrammeters.

$R =$ resistence in the conductor expressed in ohms.

$I =$ intensity of current expressed in ampères.

$E =$ electro motive force expressed in volts.

No formulas can express quantity of work without representing the three simple elements—*force, velocity* and *time*, and as the time is wanting in Count Du Moncel's formulas, they cannot express work.

In the simplest formula for work, namely, $K = FS$, or the product of force and space, the time is included in the space S, which is the product of velocity and time. No work can be accomplished without time.

Work is transformed into power by eliminating the time, and the power is thus expressed "work per unit of time," and therefore no work can be transformed into power without dividing it with the time in which it is accomplished, but the Count divides work by the acceleratrix of gravity, and calls the quotient *power*, namely, as follows:

$$P = \frac{RI^2}{g}. \qquad 4.$$

$$P = \frac{E^2}{gR}. \qquad 5.$$

$$P = \frac{EI}{g}. \qquad 6.$$

$P =$ power in effects, which is expressed in kilogrammeters per second.

To divide work with the constant acceleratrix of gravity $g = 9{\cdot}81$ meters per second, which is velocity, will give a quotient of momentum of time FT, when a force F acts for a time T upon a mass free to move. Therefore, the formulas 4, 5 and 6 cannot express power, or the work done per unit of time, when formulas 1, 2 and 3 mean work. It appears that the first three formulas mean *power*, which, multiplied by time T seconds, will give work.

The term *intensity* of current is evidently intended to mean a definite physical quantity, which should have a definite unit of measure, but *intensity* is applied to heat, light, and other phenomena without a definite meaning.

It is evident from the formulas, and also by the mode of measuring *intensity* of current by a galvanometer, that *intensity* means velocity of the electric current, and can, consequently, be expressed by any unit of length per unit of time. *Ampère* is the unit of measure of *intensity* of current, which can be expressed by a unit of velocity. The present value of one *Ampère* is, or should be, a velocity of 1,000 kilometers per second. Therefore, the term *intensity* should be abolished in that connection, and call it what it really is, namely, *velocity* of current, and denote the same with V instead of I, which would make the expression intelligible and definite.

Electromotive force is an expression of a definite physical quantity, which can be measured by any unit of weight, but the unit *volt* does not convey an idea of a definite unit of weight or force. The electromotive force of one *volt* is a weight or force of about one milligram. The term *volt* should therefore be abolished in that connection, and call it what it really is, namely, *milligram*, and the electromotive force should be denoted by F, which means force. These proposed changes would make the subject intelligible and definite, like in mechanics, where we have the well-known physical element *force, velocity* and *time*, the combination of which, give the functions *power, space* and *work*.

The old and new forms of electro dynamics would compare as follows :

	Old form.		*New form.*	
Power.	$P = \frac{RI^2}{g}$.	1.	$P = V^2$.	1.
	$P = \frac{E^2}{gR}$.	2.	$P = \frac{F^2}{R}$.	2.
	$P = \frac{EI}{g}$.	3.	$P = FV$.	3.
Work.	$K = RI^2$.	4.	$K = RV^2T$.	4.
	$K = \frac{E^2}{R}$.	5.	$K = \frac{F^2T}{R}$.	5.
	$K = EI$.	6.	$K = FVT$.	6.
Resistance.	$R = \frac{E}{I}$.	7.	$R = \frac{F}{V}$.	7.
	$R = \frac{gP}{I^2}$.	8.	$R = \frac{P}{V^2}$	8.
	$R = \frac{E^2}{gP}$.	9.	$R = \frac{F^2}{P}$.	9.
Electro motive force.	$E = RI$.	10.	$F = RV$.	10.
	$E = \sqrt{gRP}$.	11.	$F = \sqrt{PR}$.	11.
	$E = \frac{gP}{I}$.	12.	$F = \frac{P}{V}$.	12.
Velocity of current.	$I = \frac{E}{R}$.	13.	$V = \frac{F}{R}$.	13.
	$I = \sqrt{\frac{gP}{R}}$.	14.	$V = \sqrt{\frac{P}{R}}$.	14.
	$I = \frac{gP}{E}$.	15.	$V = \frac{P}{F}$.	15.
Time of operation in seconds.			$T = \frac{K}{RV^2}$.	16.
			$T = \frac{KR}{F^2}$.	17.
			$T = \frac{K}{FV}$.	18.

$P =$ power in effects, which is expressed by kilogrammeters per second. $R =$ resistance of conductor in ohms.

$E = F$, the electromotive force expressed in milligrams.

$I = V$, the velocity of the electric current in thousands of kilometers per second; that is, when $V = 1$, the velocity is 1,000 kilometers per second.

$T =$ time of operation in seconds. $K =$ work in kilogrammeters.

There are 75 French effects per horse-power.

There is a discrepancy between the old and new formulas for power, which is caused by Count Du Moncel dividing the work by the accelerratrix g, and calls the quotient *power*. Therefore, his formulas for work do not agree with his formulas for *power*.

The new form gives the *power* ten times greater than the old form, on account of the unit of electromotive force is assumed to be one milligram, and the unit of *velocity* 1,000 kilometers per second, which ought to be the units of these quantities.

Comparison of French and English units of power and work.

English.		French.	
Effects P.	Horse-power HP.	Effects P.	Horse power HP.
1	0·001818	0·1382	0·00181
550	1	76·2	1·0136
7·2334	0·00178	1	0·01333
512·47	0·9863	75	1

The numbers which express units of power are equal to the numbers which express units of work done per unit of time, but the quantities, power and work are different from one another, as explained above.

I beg leave to repeat what I have stated many times within the last twenty years, namely, that the science of electricity can never be cleared up before the electrical savants learn to understand dynamics. The greatest difficulty they have to encounter appears to be the distinction between *force, power* and *work*, which quantities they now confound with one another.

The difference between *force, power* and *work* is respectively the same as that between *length, surface* and *volume.*

THE ELLIPTICITY OF PLANETS.

By Pliny Earle Chase, LL. D.

Professor d'Auria's discussion of this subject, in the June number of the Journal of the Franklin Institute, is interesting, not only on account of the amount of terrestrial oblateness which he deduced, but also on account of the discrepancy between the theoretical and the observed values and the evidence, which is thus afforded, that he has not considered all the elements which are involved in the question. This is a common mistake in the theoretical discussion of physical problems. The fact of its existence should teach investigators how unsafe it is to dogmatize upon any results which they may obtain.

Newton (Principia, B. 3, Pr. 19) deduced $\frac{1}{289}$ for the equatorial centrifugal force, and about $\frac{1}{220}$ for the oblateness. The latest estimate of the actual oblateness which has fallen under my notice is Listing's (1878, cited by Newcomb and Holden in their Astronomy, p. 202), 1 : 288·5. This is about twice as great as d'Auria's theoretical value, but it agrees very nearly with Newcomb's estimate of the centrifugal force.

If proper allowance is made for Earth's orbital ellipticity, the harmonic mass-estimate of Sun ÷ Earth (this Journal, May, 1884, p. 356) becomes 329,196, and the Sun's mean distance is 92,542,800 miles. The velocity which would precisely balance the centripetal and centrifugal forces at the equator ($v_1 = \sqrt{gr}$) is 4·90743 miles per second. The actual velocity of rotation, or $v_2 =$ ·28897 miles; $v_1 \div v_2 = 16·98$; $(v_1 \div v_2)^2 = 288·4$, which differs by only $\frac{1}{29}$ of one per cent. from Listing's estimate.

The nebular hypothesis connects Neptune, the planet which represents incipient nebular subsidence in the solar system, with Earth, the chief centre of condensation, by the same ratio which connects Earth's equatorial velocity of equilibrium with its velocity of rotation; 329196 ÷ 16·98 = 19387·3. Newcomb's estimate is 19380 ± 70. Such closeness of accordance indicates æthereal activities, which are calculated to throw great discredit upon all theories of the retardation of Earth's axial rotation, by "tidal brakes" or by any other influence.

THE DISCHARGE OF TURBINE WATER WHEELS.

By J. P. Frizell

Though it is very desirable to be able to compute with reasonable exactness the quantity of water required by a projected water wheel, this subject is chiefly important in reference to rentals for the use of water. A very simple and reliable method of determining the quantity of water drawn by a lessee consists in determining, once for all, the discharge of his wheels with different openings of gate and different velocities. These results being tabulated afford a ready means of finding the discharge at any time, by observing these elements together with the head acting on the wheel. These results, however, are always obtained experimentally, by measuring the discharge by means of a weir or other known method. No engineer of reputation, to my knowledge, professes to be able to compute the discharge on theoretical grounds.

There is no reason in the nature of things why such a computation cannot be made. The discharge of a wheel is just as clearly determined by its dimensions, head and velocity, as the discharge of a weir is by its dimensions and the depth thereon. Our inability to effect the computation in the former case is due wholly to want of knowledge of the laws of hydraulics.

It has appeared to me that the method of estimating the effect of centrifugal force, given by writers on this subject, has been a great obstacle to the development of a correct theory of the motion of water in a turbine wheel. The nature of the error involved in this method I endeavored to point out in a note printed in the number of this Journal for August, 1883.

An article appears in the May number of this Journal, by Prof. I. P. Church, of Cornell University, in which he maintains, if I understand him, that the expression employed by Weisbach and other writers to represent the effect of centrifugal force in a turbine water wheel is correct, and that the error complained of is verbal rather than real. He supports this view by certain mathematical investigations, and applies the common theory to the computation of the discharge in an experiment upon a turbine in the Tremont Mills, at Lowell, Mass., made by Mr. Francis in 1851, obtaining a result which differs from that of experiment by about 16 per cent. of the latter.

Scientific controversy is probably the most unprofitable employment

of the human mind, and next to that is the exhibition of formulas in hydraulics founded upon imaginary assumptions and hypotheses. I will forbear any comment upon the mathematics of Prof. Church's article, and, accepting the suggestion implied in the above-mentioned computation, will rest the question of the accuracy of my views upon their agreement with the results of the series of experiments referred to. I will consider the experiments with full gate only, taking the turbine as I find it, without any assumption as to its relative dimension, velocity, or the nature of the motion of the water.

I will adopt, so far as necessary, the notation used by Prof. Church, *i. e.*:

Q = volume of water discharged by the wheel.
h = head acting on the wheel.
F = cross section of guide passages at exit.
F_1 = " " wheel " entrance.
F_2 = " " " " exit.
c = velocity through F, c_1 = do. through F_1, c_2 = do. through F_2.
r_1 = radius of wheel at inner ends of floats.
r_2 = do. at outer ends.
ω = angular velocity of wheel.
a = complement of the angle which a tangent to the guide at its extremity makes with a radius to the wheel at the same point.

The inner ends of the floats make substantially a right angle with the inner circumference of the wheel.

The foot and second are the units implied in the above notation.

Aside from the centrifugal force, the general theory of the motion of water in the wheel may be stated very briefly. We suppose the velocity c to be decomposed into its radial and tangential components viz., c sin. a and c cos. a.

The tangential component is inoperative as regards the discharge.

At the entrance to the wheel there is a loss of head, being the head due the difference between c_1 and the radial component of c, viz.:

$$\frac{1}{2g}\,(c \sin. a - c_1)^2 = \frac{1}{2g}\left(c \sin. a - c\,\frac{F}{F_1}\right)^2$$

There is a loss of head due the friction of the water in the supply pipe, and the guide and bucket passages. This might be arrived at by a laborious computation, but as I am not aiming at niceties I will adopt the data given by Wiesbach (Hydraulics Du Bois's trans., p. 359), viz.:

$$\text{Frictional head} = f\frac{c^2}{2g} + f_1\frac{c_2^2}{2g}.$$

Wiesbach says, "We may take $f = f_1 = 0{\cdot}05$ to $0{\cdot}10$." I put, for simplicity,

$$\text{Frictional head} = 0{\cdot}15\,\frac{c^2}{2g},$$

being about the mean given by Wiesbach.

I also adopt Wiesbach's values of the co-efficients of discharge for he guide and bucket orifices. He finds that the cross section of a stream issuing from a straight pipe is about 3 per cent. less than that of the pipe, while a very slight convergence diminishes the stream by 5 per cent. This I adopt.

As soon as the water takes part in the rotary movement of the wheel, it is acted on by a new force, for which (begging Prof. Church's pardon) I can find no better term than "centrifugal force," urging the water *from* the centre of the wheel. The next step is to determine the effect of this force.

Let us fix our attention upon any point, P, in the float curve, at a distance, r, from the centre of the wheel. Let ω_1 represent the angular velocity of the water with reference to the wheel, v its radial velocity. Then the tangential component of the water's absolute motion is represented by $(\omega - \omega_1)\,r$, the radial component by v. By the laws of centrifugal force the former is equivalent to an acceleration of $(\omega - \omega_1)^2\,r$, the latter to an acceleration of $-\,v$. The centrifugal force, therefore, acting on a mass, M, of water is

$$M\,\left\{(\omega - \omega_1)^2\,r - v\right\}$$

We must next find the head due to centrifugal force by determining the work done by the same on a given weight of water in passing through the wheel, and dividing the result by the weight. To do this by forming the polar equation of the float curve, and proceeding according to the strict methods of the calculus, would lead to a hopelessly complex expression. I prefer to resort to the more humble method of approximation by graphical measurement. At P let d represent the angle between the tangent of the float and radius of the wheel, and F_3 the section of the bucket (space between two floats) by a cylindrical surface concentric with the wheel. Then

$$v = c\,\frac{F}{F_3},\ \omega_1 r = v\ \tan.\,d = c\,\frac{F}{F_3}\ \tan.\,d.$$

The expression for centrifugal force then becomes

$$M\left(\omega^2 r - 2\,\omega\,c\,\frac{F}{F_3}\,\tan.\,d + c^2\left(\frac{F}{F_3}\right)^2\tan.^2 d - c\,\frac{F_1}{F_3}\right)$$

Dividing the bucket into any number of parts by concentric cylindrical surfaces, we can compute the coefficients of ω, ω^2, c, c^2 for each part.

By the kindness of Mr. James B. Francis, I have been permitted to examine and measure the original full-size drawings of the wheel, now on file in the office of the Proprietors of the Locks and Canals on Merrimack River, at Lowell, Mass., from which I obtain the following values:

$r_1 = 3{\cdot}375$ ft. $r_2 = 4{\cdot}146$ ft.

$F = 6{\cdot}53$ sq. ft. $F_1 = 19{\cdot}35$ sq. ft. $F_2 = 7{\cdot}467$ sq. ft.

$a = 19°\ 5'$.

From these, together with the several values of d and F_3, I construct the following table:

Radius r. Inches.	Distance from inner edge of crown, measured on radius of wheel. Inches.	F_3 = area of float passage or bucket, measured on cylindrical surface, of radius r, concentric with wheel. Square feet.	$\frac{F}{44\,F_3}$ = numerical coefficient of c in the expression for the radial velocity.	d = angle between tangent of float and radius of wheel.	$\frac{F}{44\,F_3}$ tan. d = numerical coefficient of c in the expression for the relative tangential velocity of the water.	Numerical coefficient in the expression for the centrifugal force of ω^2.	of ω.	of c^2.	of c.
41	0·5	0·4397	0·3374	4° 27′	0·0263	3·417	0·0526	0·0002	0·3374
42	1·5	0·4418	0·3359	13° 43′	0·0820	3·500	0·1640	0·0019	0·3359
43	2·5	0·4430	0·3350	23° 20′	0·1445	3·583	0·2890	0·0058	0·3350
44	3·5	0·4460	0·3328	32° 47′	0·2143	3·667	0·4286	0·0125	0·3328
45	4·5	0·4506	0·3294	39° 55′	0·2756	3·750	0·5512	0·0202	0·3294
46	5·5	0·4564	0·3252	48° 37′	0·3691	3·833	0·7382	0·0355	0·3252
47	6·5	0·4639	0·3199	54° 41′	0·4515	3·917	0·9030	0·0520	0·3199
48	7·5	0·4739	0·3132	62° 06′	0·5915	4·000	1·0830	0·0875	0·3132
49	8·5	0·4868	0·3056	69° 38′	0·8232	4·083	1·6464	0·1660	0·3056
49·625	9·125	0·5064	0·2931	77° 18′	1·3006	4·135	2·6012	0·4091	0·2931
49·75	9·25	....	....	Extremity 78° 27′					
Sum of the first nine coefficients and ¼ of the 10th......						34·784	6·5063	0·4839	3·0077
$\frac{1}{12}$ of do......						2·899	0·5122	0·0403	0·2507

The centrifugal force is represented by the expression

$$\left(M = \frac{W}{g}\right)(34{\cdot}784\,\omega^2 - 6{\cdot}5063\,c\omega + 0{\cdot}4839\,c^2 - 3{\cdot}0077\,c),$$

in which M is the mass and W the weight of water included between two consecutive sections one inch apart; $\frac{1}{4}$ the value of the last coefficient is taken because this applies only to $\frac{1}{4}$ inch. The expression may also be understood to represent, in inch-pounds, the work done by centrifugal force on the said mass while passing through the wheel; hence the division by 12 to reduce to foot-pounds. The head due the centrifugal force is represented by

$$\frac{5{\cdot}798\omega^2 - 1{\cdot}0844\omega c + 0{\cdot}0806c^2 - 0{\cdot}5014c}{2g}.$$

This expression in the formula for discharge takes the place of

$$\frac{\omega^2\,(r_2^{\,2} - r_1^{\,2})}{2g},$$

given generally by writers on the turbine.

The principles stated in what precedes may be expressed algebraically as follows:

$$\frac{c_2^{\,2}}{2g} = h - \frac{c^2}{2g}\cos.^2 a - \frac{c^2}{2g}\left(\sin.\,a - \frac{F}{F_1}\right)^2 - \frac{0{\cdot}15\,c^2}{2g} +$$

$$\frac{1}{2g}(5{\cdot}798\omega^2 - 1{\cdot}0844\omega c + 0{\cdot}0806c^2 - 0{\cdot}5014c),$$

or

$$\left(c\,\frac{F}{F_2}\right)^2 2gh - c^2\cos.^2 a - c^2\left(\sin.\,a - \frac{F}{F_1}\right)^2 - 0{\cdot}15c^2 +$$

$$5{\cdot}798\omega^2 - 1{\cdot}0844\omega c + 0{\cdot}0806c^2 - 0{\cdot}5014c.$$

Or, substituting numerical values, and reducing

$$1{\cdot}7277c^2 = 2gh + 5{\cdot}798\omega^2 - 1{\cdot}0844\omega c - 0{\cdot}5014c,$$

and

$$Q = 0{\cdot}95Fc.$$

The results given in the following table are computed by this formula. They are arranged according to the ascending values of ω. The table embraces in reality the entire series with full gate, the experiments omitted being substantially nothing more than repetitions of those included:

Number of the experiment in Mr. Francis's series.	n. Number of revolutions of the wheel per second.	$\omega = 2\pi n$. Angular velocity of the wheel. Ft. per sec.	h. Head acting on the wheel. Feet.	Q. Discharge in cub. ft. per sec.	
				By experiment.	By computation.
43	0·	0·	12·797	135·65	134·51
42	0·45431	2·8534	12·948	133·43	133·70
41	0·53232	3·3447	12·977	133·75	134·34
40	0·60000	3·7699	12·973	134·80	134·91
39	0·64702	4·0653	12·963	135·34	135·35
36	0·69471	4·3650	12·944	136·49	135·83
35	0·74211	4·6628	12·939	137·71	136·47
34	0·78401	4·9261	12·941	138·09	137·11
32	0·83624	5·2542	12·915	138·27	137·85
29	0·86643	5·4439	12·906	138·51	138·34
21	0·90201	5·6675	12·899	139·90	138·97
18	0·94507	5·9380	12·880	140·47	139·73
16	0·99945	6·2797	12·890	141·98	140·94
15	1·02373	6·4323	12·888	142·04	141·46
14	1·06744	6·7069	12·856	142·52	142·33
11	1·12518	7·0697	12·819	143·91	143·57
10	1·18460	7·4431	12·800	144·87	145·01
9	1·24514	7·8234	12·777	146·02	146·57
8	1·30933	8·2268	12·720	147·29	148·17
7	1·38249	8·6864	12·696	149·47	150·27
6	1·46149	9·1828	12·653	152·27	152·61
5	1·53218	9·6270	12·611	154·39	154·78
4	1·59651	10·0313	12·554	156·65	156·77
13	1·78404	11·2095	12·510	163·43	163·47

VENTILATION OF SEWERS.—Roger Constantin has presented a note to the French Academy, in which he claims that the health of Paris requires the establishment of ventilating chimneys for the sewers. When the note was read, Dumas stated that while the declivity of the sewers ran from the city towards the river, the air which is heated by contact with the water tends to return towards the centre of the city through the entrances of the sewers. Constantin proposed four large chimneys at the lower openings of the sewers, carried to a sufficient height to preserve the city from all infection. Berthelot thought it indispensable that the deleterious gases should be burned by passing through a fire. Casalonga suggests that it would be better to place the ventilating chimneys at the upper rather than at the lower part of the sewers.—*Chron. Industr.*, Oct. 7, 1883. C.

THE IRIDIUM INDUSTRY.

By Wm. L. Dudley, Cincinnati, O.

[Read at the Cincinnati Meeting, American Institute of Mining Engineers, Feb., 1884.]

It is my desire to call attention to a new industry which was started about four years ago, through the discovery by Mr. John Holland, a resident of this city, of methods suitable for working the metal iridium. This metal has been known since the year 1803, having been discovered by Smithson Tennant while investigating the metallic residue which remained when platinum ores were dissolved in aqua regia. The metal is classed among the rare metals, as it is not found in large quantities, although it is quite widely distributed geographically. It is found in California, Oregon, Russia, East India, Borneo, South America, Canada, Australia, and in certain parts of France, Germany, and Spain. The principal sources of supply of the metal are Russia and California; it is nearly always found with either platinum or gold, is extracted from those ores as a by-product, and is always found in small grains or fine powder, the largest pieces being about the size of a grain of rice. In nature it is generally alloyed with other metals, and the two metals with which it is most commonly alloyed are platinum and osmium; the platinum alloy is called platin-iridium, and the osmium alloy osmiridium, or iridosmine. The grains of platin-iridium are sometimes found as small cubes with rounded edges, while the iridosmine usually exists in the form of flat irregular grains and occasionally as hexagonal prisms. The supply of metal which is derived from Russia is generally obtained from the platinum mines which are situated in the Ural Mountains; while in California it is found principally in the placer gold-washings. The ores of iridium are a source of great annoyance when mixed with gold-dust on account of its specific gravity, which is about 19·3, being nearly the same as that of gold. Consequently, it is impossible to separate the gold from the iridium by the process of washing; the separation may, however, be made either by the amalgamation of the gold (as neither iridium nor its ores combine with mercury), or by dissolving out the gold in aqua regia.

In the mints these metals are frequently separated by melting the gold-dust and allowing the molten mass to remain in the crucible for some time, during which the iridium slowly settles to the bottom as it

does not alloy with the gold under such circumstances. The gold is then poured off from the top, and the dregs in the bottom of the crucible are found to contain the greater quantity of the iridium. The gold contained in the dregs is then dissolved and the iridium is found in the residue.

In Russia it is contrary to the law to possess or deal in iridium ore, and, consequently, the government takes possession of all the ore which is mined or extracted from the platinum ores in that country, and it is stored in the vaults of the Royal Mint. The reason for the law is, that some years ago the Russian government found that speculators in gold-dust would sometimes add iridium to it (which often escaped detection by the officials of the mint), for the purpose of increasing its weight. On attempting to work this gold they found the fine particles of iridium distributed through the ingots; on rolling the ingots into sheets, these individual grains or particles produced indentations in their steel rolls; and in striking out the coins the dies were marred and defaced thus causing considerable loss to the government. In order to prevent this species of fraud, the government passed a law prohibiting persons from handling the metal in any way and compelling its immediate surrender.

For a number of years experimenters have endeavored to melt the iridosmine or iridium in order to utilize it in the arts for such purposes as could with advantage employ a metal of its wonderful properties; but until lately success has been only partial.

I may here briefly describe the properties of the metal itself. Iridium possesses a white lustre, resembling that of steel; its hardness is about equal to that of the ruby; in the cold it is quite brittle, but at a white heat it is somewhat malleable. It is one of the heaviest of metals, having a specific gravity af 22·38. When heated in the air to a red heat the metal is very slowly oxidized; but upon raising the temperature to about 1,000 C., it parts with its oxygen, and hence at a high heat (above 1,000 C.), it is not oxidized. It is insoluble in all single acids, but is very slightly soluble in aqua regia after being heated in the state of fine powder for many hours. In a massive state, however, aqua regia does not attack it.

In attempting to fuse iridium heretofore, it has been found by experimenters that the best results that could be obtained were by means of the oxy-hydrogen or electric furnaces, but that with either of these means of fusion they could only work a very small quantity of the

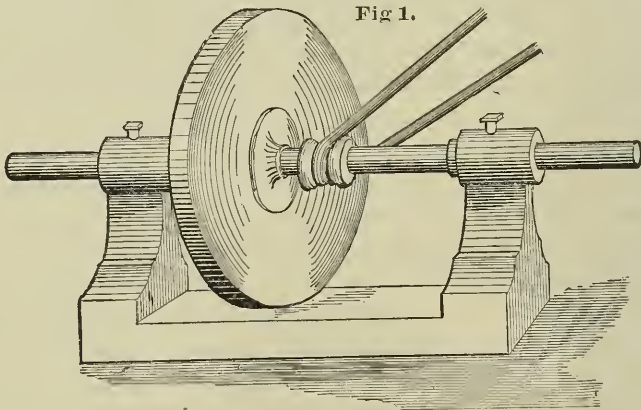
metal at a time, and obtain a globule of very small size. Previous to the present time iridium has had substantially but one use (with the exception of alloying with platinum), viz., for pointing gold pens. The iridium point is commonly called "diamond point" upon a gold pen, and it consists simply of a small grain of iridosmine which has been selected for the purpose, and soldered to the tip of the pen. These are selected by first removing from the ore, with a magnet, the magnetic oxide of iron which always accompanies it, and then dissolving out, by means of acids, the other impurities which may be present; the ore is then washed with water, dried and sifted in order to remove the fine dust, and the sifted ore is then ready for the selection of points. This is done by an operator, who rolls the grains of iridium around with a needle point, examining them under a magnifying glass, and selecting those which are solid, compact, and of the proper size and shape. These points are usually selected in three grades, small, medium, and large, depending upon the size of the pen for which they are intended to be used. The grain of iridium having been soldered on to the end of the pen, it is sawed in two (which makes the two nibs of the pen), and ground up in the proper shape.

About four years ago Mr. John Holland, the well known gold pen manufacturer, found it necessary to have pieces of iridium larger than those generally found in nature for the purpose of making points for the Mackinnon stylographic pen. After many experiments he found that by heating the ore in a Hessian crucible to a white heat and adding to it phosphorus, and continuing the heating for a few minutes, he could obtain a perfect fusion of the metal, which could be poured out and cast into almost any desired shape. This material was found on physical examination to be about as hard as the natural grains of iridium; and in fact seemed to have all the properties of the metal itself. On chemical analysis it was found that the metal fused with phosphorus contained, according to two determinations, 7·52 per cent. and 7·74 per cent. of phosphorus. At this stage of the discovery we became acquainted with it, and began experiments with the intention of putting the product to some practical use in the arts. It was found that the presence of phosphorus rendered the metal quite readily fusible at a white heat, but this, of course, was an obstacle in the way of its use for electrical purposes. Desiring, therefore, to remove the phosphorus, we found by experimenting that by heating the metal in a bed of lime the phosphorus could be completely removed. In this operation, the

metal is first heated in an ordinary furnace to a white heat, and finally, after no more phosphorus makes its appearance, it is removed and placed in an electric furnace with a lime crucible and there heated until the last traces of phosphorus are removed; the metal which then remains will resist as much heat without fusion as the native metal.

In mechanical applications, where the metal is not subject to great heat, it is melted with phosphorus and cast into the shape desired, and then ground or worked as the application may require. The first application to which it was put was for the manufacture of the Mackinnon pen-points. For this purpose, the metal, after being fused, is removed from the furnace and poured between two slabs of iron which are kept apart the desired distance so as to make a sheet of iridium of

Fig 1.



the thickness required. The metal is poured, as I have said, between these plates, and the plates are brought suddenly together, on the plan of a closed ingot with a hinge, so that as the metal cools it is subjected to pressure which closes the pores and makes a very compact casting. The slabs for the Mackinnon pen-points are about one thirty-second of an inch in thickness, and are broken up into small irregular pieces which are soldered on a strip of brass and ground down to a flat surface by means of a copper-lap. The copper-lap (Fig. 1) consists of a plate of copper, about one-half inch in thickness and eight inches in diameter, fixed on a spindle which is made to revolve from eight hundred to a thousand revolutions per minute; the copper of which the lap is composed is wrought copper, well annealed, and consequently

very soft. In order to grind with it, corundum or diamond-dust is mixed with oil and applied to the flat surface of the lap by means of a flat steel instrument, upon which pressure is applied in order to force the corundum or diamond-dust into the copper, thereby making a cutting surface. The irridum to be ground is held against this sharp surface of the lap, and the corundum or diamond-dust gradually cuts the metal away. As the cutting material wears from the copper-lap, another application of the corundum and diamond-dust is made by

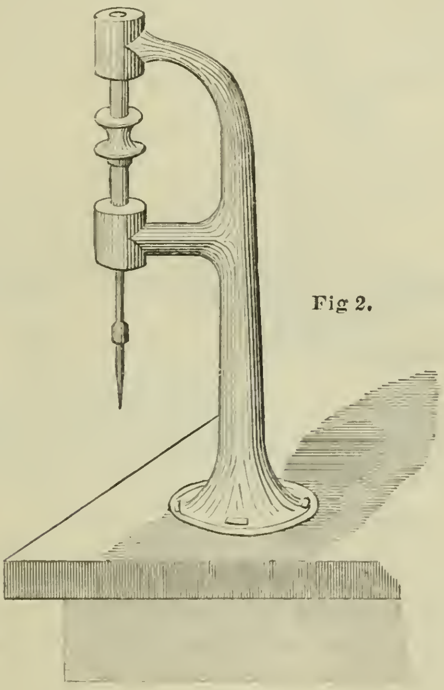
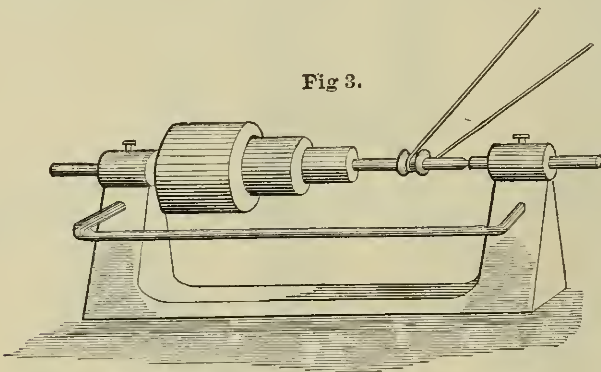


Fig 2.

means of the steel instrument as described; this operation is continued until the grinding is complete. After the slabs are ground to a surface they are then drilled. In the drilling operation, the iridium is first countersunk by means of a diamond drill, consisting of an upright spindle suitably fixed in a frame so as to revolve freely; the bottom of the spindle holds a small rod of brass, to the lower end of which is set a white diamond-splint. This drill is made to revolve about nine hundred revolutions per minute. The iridium is held up against the

diamond with a light pressure, and the diamond gradually makes a conical hole or countersink. After countersinking the iridium, it is finally pierced by means of a copper drill (Fig. 2), which consists of a piece of soft copper wire, which is filed down to a point and set in a drill similar to that in which the diamond is placed, but this drill makes about thirty-five hundred revolutions per minute. Corundum or diamond-dust and oil is put into the countersink opening in the iridium, and then it is held up against the piece of revolving copper. The diamond-dust or corundum, imbedding itself in the copper, acts as a cutting surface, and finally accomplishes the drilling of the hole. The holes having been drilled in the pieces of iridum which were soldered to the brass, the brass is finally dissolved from the iridium by

Fig 3.



means of nitric acid; and then we have irregular-shaped pieces of iridium, pierced with holes. These pieces of iridium are then soldered in proper position to the end of the Mackinnon pen, fitting into the opening of which there is a valve consisting of an iridium-pointed wire. The iridium is then ground to the proper shape on the outside by means of a copper-lap, as shown in (Fig. 3), consisting of three or more copper cylinders on a common spindle, making about three thousand revolutions per minute.

The operation of sawing iridium is carried on by means of a copper disk (Fig. 4), from four to eight inches in diameter, made of soft thin sheet-copper, held between two clamps, placed on a spindle revolving at the rate of about twenty-five hundred revolutions per minute. This sheet of copper revolves in a box which contains corundum or diamond-dust and cotton-seed oil. The cotton-seed oil with the cut-

ting material adheres to the periphery of the saw, and as the saw comes in contact with a piece of iridium it gradually does the work. Cottonseed oil is preferred for this purpose to any other oil, on account of its viscosity.

I may mention briefly some of the uses to which iridium has been applied. First, I will refer to the draw-plate for drawing wire. There are at present, besides the iridium draw-plate, the ordinary steel plate which is used for drawing heavy wire, and the ruby plate, which is used in drawing gold and silver wires, where it is desirable to have

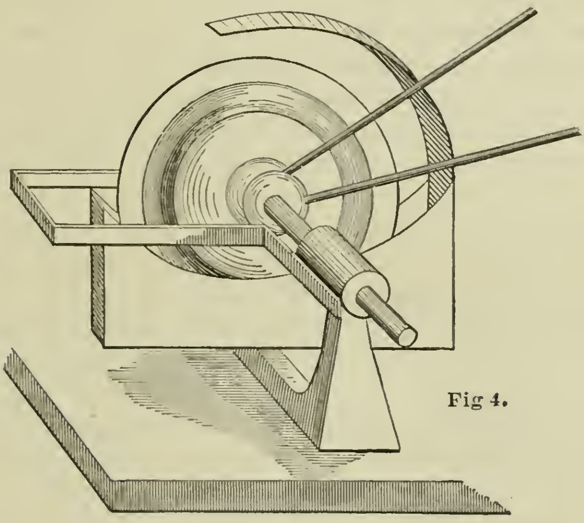


Fig 4.

them of uniform thickness. The iridium plate is made somewhat similar to the ruby plate, consisting of a piece of iridium which has been countersunk and drilled in the usual way to the size of the hole required, and set in a brass plate, where it is firmly held by a bushing. This plate is now coming into use, and is rapidly taking the place of the ruby plate, being equal to the ruby in hardness and much more durable, since it is less liable to break or chip by rough handling or heating.

Iridium knives are made for fine scales and balances, the bearing edge of which consists of iridium, soldered firmly to a brass body. These are rapidly taking the place of the agate for fine chemical balances, and there seems to be no reason why they should not have even a more extended use, since they are superior to the agate in that

they take a finer edge and thereby make a more delicate balance, and are not so liable to crack or break. They are now being used altogether by Mr. Henry Troemner, of Philadelphia, the well-known scale manufacturer, for the purpose of adjusting his weights for all of his scales.

Hypodermic needles for physicians' and surgeons' use are now made of gold and tipped with iridium, in place of the old steel pointed ones, which are liable to rust or corrode if not properly taken care of. The iridium being hard will take a good edge, and is not subject to corrosion, as is steel.

Styluses for manifold writing are also being made with iridium points, having decided advantages over either steel or agate. Iridium points are also being applied to surveyors' and engineers' instruments, and in all places, in fact, where hardness, durability, and non-corrosibility are required. For all the above uses the iridium alloyed by fusion with phosphorus is employed.

Some years ago experiments were made in order to apply this metal to the electric light. We found that an iridium electrode used upon the negative of an arc-light would keep its shape and resist the heat, provided the positive carbon which was used with it was not allowed to strike or fall too heavily upon the iridium negative. Since the metal at a white heat becomes malleable, a continual pounding or striking would gradually beat the negative out of shape. The iridium negatives are made by setting a piece of de-phosphorized iridium in the end of a brass rod about six inches long and nine-sixteenths of an inch in diameter. The length of the iridium is about half an inch, ground conical in shape. It was found that the brass, being only half an inch from the arc, would resist the action of the heat; but in some cases where the lamp flamed the brass was liable to undergo partial fusion; and in such cases it was found desirable to put a thimble or cap of platinum over the end of the brass and just below the iridium, the platinum thimble being about half an inch long.

One of the most important applications of iridium which has yet been made is to the electrical contact-points of telegraphic apparatus. These contact-points consist of pieces of copper wire tipped with iridium, which are set in the instrument just as platinum points are set. These contacts will outlive many platinum contacts; are not subject to oxidization or sticking as are the platinum ones; and all that is necessary in order to clean them when they become dirty is to pass over

their surface an emery file or a piece of fine emery-paper. These contacts have been thoroughly tested by various eminent electricians and also by long continued use, and the advantages herein stated have been in every case fully demonstrated.

In the past three years we have been experimenting on methods of plating with iridium, and about one year ago we succeeded in obtaining a bright reguline deposit of iridium on base metals. This deposit resembles the natural metal, being quite hard and resisting the action of acids. There were many difficulties encountered in accomplishing this result, on account of the power of the metal to resist the action of the solvents; but we have succeeded in obtaining a solution which gradually attacks an anode of iridium, and it is hoped in a short time that all the minor practical difficulties will be overcome in the plating of articles on a commercial scale. So far, we have been thoroughly testing our results, and do not feel prepared to place our work before the public until it is perfect in all its details.

DISCUSSION.

JAMES C. BAYLES, New York City:—I would like to ask Mr. Dudley how the anode is prepared in electro-plating; whether from iridium in grains, or in the fused state.

MR. DUDLEY:—We use iridium which has been fused with phosphorus. It contains 5 per cent. of phosphorus, which does no damage. Of course, in working out a problem of this nature, it takes several years to ascertain all the causes of disturbance; but we have not yet attributed any trouble to the phosphorus in the solution. We have made many experiments with alloys of iridium, but have practically failed to get an anode of them which would readily dissolve; but we find that the anode of fused iridium gradually dissolves, and yet not fast enough to keep the solution replenished, without the addition, now and then, of some of the pure salt, the amount depending upon the kind of solution employed. The same thing is necessary in silver, and in nickel-plating. Theoretically, the anode should dissolve and keep the solution up to the proper strength. In practice, that is never the case.

MR. BAYLES:—How is the solution made?

MR. DUDLEY:—This solution is made by dissolving the iridium first with chlorine and sodium chloride. The iridium or iridosmine is mixed with common salt, placed in a tube, and heated to redness. Chlorine gas is then allowed to flow slowly through the tube for several

hours, at the end of which time most of the iridium will have combined with the salt and chlorine to form the double chloride of iridium and sodium, which rapidly dissolves in water. From this solution we can obtain any salt of iridium which we desire. In plating with the metal, we find that a solution, very slightly acidified with sulphuric acids, gives the best results practically, although neutral or alkaline solutions work very well.

A MEMBER:—What kind of solder is used in soldering iridium to other metals?

MR. DUDLEY:—Ordinary silver-solder. Soft solder will solder the metal as well, but we seldom use it, since it is not as durable.

DR. R. W. RAYMOND, New York City:—I would ask whether it is known what is the possible supply of that metal.

MR. DUDLEY:—Well, we find it, like a good many other metals, widely distributed—in Canada, and many parts of the United States, in France, and a great many European countries, and, as I have said, the largest supply has heretofore been in Russia. The Russian government is very arbitrary, and they enforce their laws right up to the mark, and generally when they make a law, they make it pretty strong to start with. (Laughter.) As iridium was found principally in Russia, we sent over a representative to see what could be done about getting our supply from there. He went to the United States consul at St. Petersburg, and was informed that there was a law, requiring persons who had any iridium in their possession, to turn it over immediately to the authorities, on pain of being exiled to Siberia for life. (Laughter.) The result was that, in a short time, the mint had stored up in its vaults quite a supply of the metal. Our representative went to the Consul, who telegraphed to the platinum mines, where most of the iridium is obtained, but received no answer. He wrote a letter, but still no answer. Telegraphed again. No answer. He finally found that as every telegram of communication was liable to be opened and read, those receiving the same were afraid to have anything to do with the matter for fear of going to Siberia. The result was that he could do nothing. But there are some parties in Hamburg, who deal in the ore, and we get most of it from them. It is said that they get it from the mint at St. Petersburg. I may say, also, that we get from three to four hundred ounces annually from the United States government. We do not get more for the reason that the mints will not knowingly buy gold-dust which contains any iridium. They examine the dust very carefully, but, in spite of their care, they get a little.

PHYSICAL AND CHEMICAL TESTS OF STEEL FOR BOILER- AND SHIP-PLATE FOR THE UNITED STATES GOVERNMENT CRUISERS.*

By Pedro G. Salom,

Chemist and Superintendent of Steel Department, Chester Rolling Mills, Thurlow, Pa.

I have had an opportunity, within the last few months, of making a large number of physical and chemical tests of steel for boiler and ship-plate, which has been, and is used principally for the United States Government cruisers, now building in Mr. John Roach's yard, at Chester, Pa. Through the courtesy of the Messrs. Houston, of the Chester Rolling Mills, I have the honor to lay before the members of the Institute the results of these tests. These results have developed some points in regard to the nature of steel, which are more or less new, and which, I hope, will prove interesting.

The original specifications, as regards the *manner* of testing, were so impracticable (when their severity was taken into consideration), that the keel of the first vessel, which will be launched next month, would, had they been carried out, probably not have been laid until next year. At my suggestion, in order to simplify and expedite the immense amount of testing that had to be done, the Naval Advisory Board was induced, after considerable discussion, to alter the specifications, so as to test each heat, instead of each lot of twenty plates, but reserved the right of making the quenching-test on a piece from each ingot, after it had been rolled.

This relieved us from the anxiety of having a lot of plates rejected, after they had been rolled and sheared, by reason of the presence of one or two plates, which might have been injured in heating or rolling, or which might not have been able to stand the tests, while it insured the Government in obtaining a uniform quality of steel, and prevented the temptation to introduce a half-dozen or more bad plates in a lot, and run the risk of their being selected as the test-plates.

I give below a portion of the original specifications, as prescribed by the Naval Advisory Board, in order to insure the fulfillment of the clause of the Act of Congress of August 5, 1882: "Such vessels . . . .

* Read at the Cincinnati Meeting, American Institute of Mining Engineers, February, 1884.

to be constructed of steel, of domestic manufacture, having, as near as may be, a tensile strength of not less than sixty thousand pounds to the square inch, and a ductility in eight inches of not less than twenty-five per centum."

RULE III.—In every lot of twenty plates, test-pieces to be cut from two plates taken at random; two test-pieces being cut from each plate,—one in the direction of the rolling, and one at right angles to it, shaped according to the annexed sketch. These test-pieces shall in no case be annealed.

The test-pieces to be submitted to a direct tensile stress, until they break and in a machine of approved character.

The initial stress to be as near the elastic limit as possible, which limit is to be carefully determined by the inspector in a special series of tests. The first load to be kept in continuous action for five minutes. Additional loads to be then added at intervals of time, as nearly as possible equal, and separated by half a minute; the loads to produce a strain of 5,000 pounds per square inch of original section of the test-piece, until the stress is about 50,000 pounds per square inch of original section, when the additional loads should be in increments not exceeding 1,000 pounds.

An observation to be made of the corresponding elongation measured upon the original length of eight inches.

Conditions of Acceptance.—In order to be accepted, the average of the four test-pieces must show an ultimate tensile strength of at least 60,000 pounds per square inch of original section, and a final elongation in eight inches of not less than 25 per cent.

Lots of material, which show a strength greater than 60,000 pounds per square inch, will be accepted, providing the ductility remains at least 23 per cent.

Cases of Failure,—If the average of these four test-pieces, numbered 1, 2, 3, 4 (called Test I), fall below either of the required limits, the plates, from which pieces 1, 2, 3, 4 were cut, shall be rejected, and Test II made, consisting of pieces 5 and 6, cut from a third plate. If the mean of the results of these two fall below either of the above limits, the entire lot shall be rejected. If it be successful, Test III, or the mean of pieces 7 and 8 cut from a fourth plate, shall decide.

If, in any of Tests I, II, III, any single piece shows a tensile strength less than 58,000 pounds, or a final elongation less than 21 per cent., the plate, from which it was cut, shall be rejected, and that Test considered to have failed, regardless of its average.

RULE IV.—*Quenching Test.*—A test-piece shall be cut from *each* plate, angle or beam, and, after heating it to a cherry-red, plunged in water at a temperature of 82°F. Thus prepared, it must be possible to bend the pieces under a press or hammer, so that they shall be doubled round a curve, of which the diameter is not less than one and a half times the thickness of the plates tested, without presenting any trace of cracking.

These test-pieces must not have their sheared sides rounded off; the only treatment permitted being taking off the sharpness of the edges with a fine file.

RULE VII.—*Each* boiler-plate must be subjected to the same tests, and in the manner prescribed for ship-plates. The ductility in eight inches must not be less than 25 per cent., and the ultimate tensile strength must not be less than 57,000 pounds, and not more than 63,000 pounds, and the average, at least, 60,000 pounds.

The following diagrams represent, Fig. 1, the 8-inch and Fig. 2, the 1-inch test-piece. The tests were made on an improved 100,000 pound Riehlé hydraulic machine.

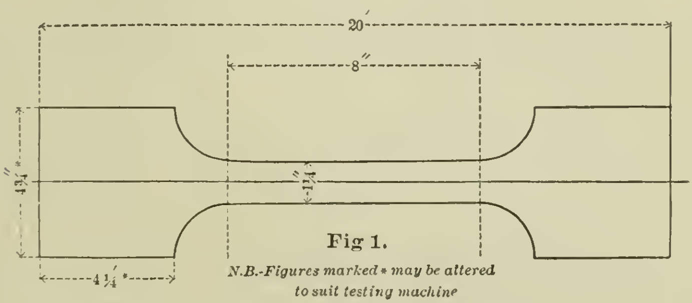


Fig 1.

In the original specifications, it will be observed by reference to the above rules, that plates of only 58,000 pounds tensile strength, might pass, provided the average of all the test-pieces was 60,000 pounds, or over; but, in the amended specifications we were not granted even this

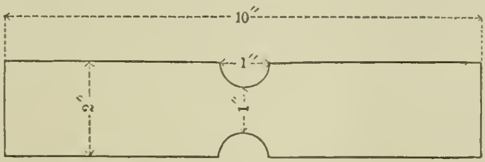


Fig 2.

little piece of grace, and, unless the average of the two test pieces, selected from each heat, was 60,000 pounds or over, and 23 per cent. stretch or over (and in neither test to fall below 58,000 pounds, or 21 per cent.), the heat was rejected. In other words, to take a practical example, if two test-pieces from the same heat gave 59,900 pounds tensile strength, with the required elongation, the heat was rejected. If, however, one of those pieces gave 58,000 pounds, and the other 62,000 pounds, with other conditions remaining the same, the heat was accepted.

To give some idea of the manner in which the inspection was first conducted, I refer to heat 435 in the accompanying table. Here the tensile strength was 56,900, instead of 57,000 (for boiler-plate), with a splendid elongation of 28 per cent.; yet, notwithstanding this fact, the heat was rejected, and we were not allowed to roll that metal for the cruisers. This, also, in view of the well-known fact that the difference of the $\frac{1}{100}$ of an inch in taking measurements of test-pieces makes a difference of 1,200 pounds to the square inch in a half-inch section,—that is, a section from a plate half an inch thick; and further, that two pieces from the same plate, taken side by side, will frequently vary several thousand pounds. If we take heat 500, with dimensions 1 × ·5, and change the ·5 to ·49, we get a tensile strength of 64,300 pounds, or 1,300 pounds difference. Conversely, if the error should be the other way, ·51 instead of ·5, we would get a tensile strength of 61,800 pounds, or 1,200 pounds difference. Of course, in plates less than half an inch in thickness, the difference is still greater.

We found no difficulty in getting the desired ductility or elongation, since all our best boiler-plate gives from 28 to 30 per cent. in an 8-inch section; but the trouble came in getting the tensile strength to run over 60,000 pounds, and maintaining at the same time the very high percentage of elongation required. This difficulty was finally overcome, and we were able to meet the specifications with gratifying regularity.

Heat 453 (see table) is inserted to show the character of steel we were making previous to our undertaking to make the cruiser-steel. The manganese, however, is a little below the average, which should be about 0·25 per cent. In other respects its characteristics are those of the best boiler-plate.

Out of the first 33 heats, from 464 to 496, we lost 14, or 42 per cent. Only four of these heats, however, could be called inferior; the remaining ten being, in my opinion, better material than that accepted. In the next 100 heats only 10 failed to pass the specifications. Nine of these were condemned on account of being too soft, only one (No. 525) for being too hard, *i. e.*, not giving the required stretch. It would have been possible to secure a still lower percentage of condemned heats; but we did not care to risk making metal like No. 525, and preferred to fail occasionally by making it too soft, since this metal, when rejected, we could use for the best flange and fire-box plate. In the last 47 heats only three failed to come up to the specifications. No. 602 we did not test, and No. 604 was lost.

The first hundred heats, from No 464 to No. 563, in addition to the usual amount of pig iron, scrap, and shearings, contained charcoal-blooms and muck-bar (gradually increased amounts of muck-bar and a corresponding diminution of blooms, which were at first charged in equal amounts.) Nos. 564 to 619, were all made from muck-bar, with results equally if not more satisfactory than with blooms. Of the remaining heats, a few were made from all blooms, a few from all muck-bar, and the others from a mixture of the two.

The tests marked with an asterisk were made on pieces cut at right angles to the direction of rolling; the others with the grain, or in the direction of the rolling.

Let us now consider the indications here given as to the influence of carbon, manganese, phosphorous, and silicon on the physical qualities of the steel. The average amount of phosphorus is ·039 per cent., and this, I think, will compare favorably with any other steel that has ever been made, taking the number of heats into consideration. The highest phosphorus is found in heats Nos. 551 and 552, where it is ·075; and yet those heats give excellent results. Again, in Nos. 548 and 549, we find ·069 phosphorus, with good results. Conversely, in heats Nos. 605 and 606, we find low phosphorus, ·042 and ·044 per cent. respectively, with results not so good; and still again, in heat No. 499, the phosphorus is only ·031, with results not equal to those first mentioned. We are safe in assuming, therefore, so far as the above tests are concerned, that phosphorus up to the amount of ·075 is not very injurious to the physical properties of soft steel. In all cases in this table where the results are not up to the standard the cause can be directly traced to something else besides phosphorus. In my opinion, phosphorus is not the terrible *bête noir* it has been considered; it has had to bear the burden of many sins wrongfully ascribed to it.

The average amount of carbon is ·15, which I regard as being from ·03 to ·05 too high for the best boiler-plate. The influence of carbon is more certain and regular than that of phosphorus, although there appear to be a few exceptions to the rule that tensile strength increases with carbon. Heat No. 517 has ·22 carbon, with 66,000 tensile strength, while heat No. 530, with ·18 carbon, gives 70,000 tensile strength, with the same amount of manganese in both cases; and heat No. 532, with ·17 carbon and less manganese than the others, gives a still higher tensile strength of 72,000. Again, in heat No. 566, we have ·20 carbon and only 59,600 tensile strength, with a splendid elongation amount-

ing in one case to nearly 31 per cent. These, however, are, I believe, the only exceptions of importance to the rule that tensile strength increases with the amount of carbon; and I will presently suggest an explanation that will account for nearly all the anomalous results.

The average amount of manganese is ·38 per cent. Manganese plays a part similar to carbon, only in a lower degree, and may have comparatively much wider limits of variation than carbon without altering the result. The highest manganese we find in heats Nos. 603 and 503, viz.: ·73 per cent. and ·58 per cent. respectively; and in both cases comparatively high tensile strength and diminished elongation; the carbon in both cases being above the average. There are other cases, however, with manganese above the average, where neither the tensile strength nor the elongation is unduly affected. Heat No. 509 is an example, but it is low in carbon. In heat No. 515, with manganese the same as in No. 509, but with carbon a few points higher, the tensile strength is high again. The lowest manganese is ·17 per cent. in heat No. 453, with the best results; and again, Nos. 538, 544, 557, and 567, showing ·24, ·27, ·29 and ·27 per cent. of manganese respectively, all give excellent results. The manganese, as we would naturally expect, increases or diminishes with the carbon, and the tensile strength in direct proportion to both.

Another point of interest to which I wish to call attention, is the variation of carbon before and after the addition of ferro-manganese. This variation ranges from ·01 per cent. in heat No. 487 to ·10 in No. 617. Assuming that ferro-manganese contains about 5 per cent. of carbon, then the theoretical amount added would be, in a one per cent. charge, just ·05 per cent. It easy to understand that the increase of carbon is sometimes less than ·05 per cent. after the addition of the ferro-manganese, because there is always more or less opportunity for oxidation, but why it should ever be greater than ·05 per cent. is not so easy to understand. I would suggest as an explanation of this abnormal increase of carbon, that a portion of the carbon, before the addition of ferro-manganese, was present as graphite, and that the ferro-manganese caused this graphitic carbon to combine.

The influence of silicon on steel has been but little studied. This has been due to the fact that it is generally present in such small quantities as to make it a question whether or not it has any influence on the physical qualities of the metal.

Silicon prevents carbon from combining with iron. This interesting

fact, although known in a general way by those who have studied the chemistry of pig-iron, was altogether ignored, or lost sight of in steel, until its importance was discovered by Col. Caron. Since then, it has been commented on in Mr. Ward's *Note on the Behavior of Manganese to Carbon*,* and in Mr. Alex. Pourcel's note on the same subject.†

Phosphorus, I believe, acts in a similar manner; for it is a well-known fact that high phosphorus irons will not *chill*, that is, the carbon will not combine with the iron. These facts open up a new line of thought; and I am led to the conclusion that phosphorus and silicon in steel are not hardeners, as heretofore supposed, but are injurious, first, because they have a tendency to keep the carbon in the graphitic state; and second, because they act mechanically (as phosphide and silicide of iron, sometimes, perhaps, as phosphate and silicate of iron) like graphite, to separate the particles of the metal, or in other words, to destroy its continuity or homogeneity. In fact, when we come to think of it as a question of molecular physics, it is difficult to conceive how they could ever have been regarded as hardeners. I am led to this conclusion not only by the results that I have obtained, but also by a study of Dr. Dudley's chemical and physical tests of rails.‡ This is such an important point that I may be excused for citing a few examples from Dr. Dudley's tables.

No. 911 with ·618 per cent. of carbon and 1·044 per cent. of manganese gives 69,000 tensile strength, while No. 906, with less than one half the amounts of carbon and manganese (·308 per cent. and ·462 per cent. respectively) gives the same tensile strength. Now there are only three hypotheses by which we can account for the tensile strength being the same, with such vastly different chemical compositions; we may suppose, first, that the carbon is not all combined; or, second, that the steel contains varying amounts of oxide of iron; or, third, that both these causes are combined. If the carbon in either of the above cases had been all combined (leaving out of the question, at present, oxide of iron), then it is evident that the tensile strength would not only have differed, but would have been much higher in both cases. No. 911 would have shown about 100,000 pounds, and No. 906 about 80,000 pounds. And if it should be found that the carbon

* Transactions, vol. x, p. 268.
† Transactions, vol. xi, p. 197.
‡ Transactions, vol. ix, p. 320.

was all combined, then the low tensile strength of No. 911 must be due to the presence of oxide of iron.

If the above conclusion be true, it follows as a corollary that wear is not so much a question of hardness or softness, as of homogeneity. At all events this theory will account for nearly all Dr. Dudley's anomalous results. It explains why rails from 80,000 to 90,000 tensile strength, are good (see Dudley's tables, Nos. 885 and 917), and why softer rails, from 54,000 to 69,000, are bad (see same tables Nos. 903 and 923.)

It confirms Dr. Dudley's general conclusions, though not for the same reasons. While on this subject, I may say that I agree with Mr. Pourcel, that a high manganese rail may show better wear than a low one, but not for the same reason as he gives. The manganese by removing oxide of iron, and counteracting the effect of silicon and phosphorus, in other words causing the carbon to combine, performs a valuable function in thus rendering the metal homogeneous; but if the same effect could be produced without any manganese being present, I maintain that the steel would be still better. Manganese, *per se*, is a hard, brittle metal, and there is no reason to suppose that when alloyed with iron its qualities change, and that it imparts only hardness without brittleness.

Unfortunately I have not been able to make any graphite or total carbon determinations except in heat No. 612, which I was led to examine more thoroughly chemically, on account of the phenomenal way it behaved in the furnace. It contains, besides the ·16 per cent. of combined carbon, ·15 per cent. of graphite carbon, and ·014 per cent. silicon. Of course I do not mean to contend that ·014 per cent. of silicon will account for ·15 per cent. of graphite; but be that as it may, the graphite is there, and produces the effect we would naturally expect. With a tensile strength of about 63,000 pounds, we only get 23·5 per cent. of elongation; whereas, with the tensile strength as low as that, other things being equal, we ought to get about 28 per cent. elongation. The pig-iron used in this heat (about 15 per cent. of the total charge), was a very high-silicon No. 1 pig; and the test-piece, after the heat was melted and ready for the ferro-manganese, instead of being tough, was as brittle as if the carbon had been ·5 per cent. or ·6 per cent. It was kept in the furnace three hours longer than the usual time (about 6 hours), and doctored with iron-ore and blooms, and finally cast with the above results. In this case the determination

of carbon by either the combustion or the colorimetric method alone, would not give the desired information.

The methods employed for chemical analysis were, for carbon, the color-test, dissolving a standard every time; for phosphorus, the molybdate method, weighing the yellow precipitate; for manganese, the modification of Ford's method, by dissolving in acid proto-sulphate of iron the binoxide precipitated from nitric acid solution by chlorate of potash, and titrating with permanganate of potash. The latter method, when carefully worked, I regard as the most accurate method of determining manganese.

To return to the physical tests: the next point worthy of consideration, is the relation of reduction of area to tensile strength and elongation. Steel may give a high percentage of elongation, with a comparatively low percentage of reduction of area, like the second tests of Nos. 611 and 644; or it may give a low percentage of elongation, with a comparatively high percentage of reduction of area, like Nos. 466 and 490. But the material that I regard as being best adapted for boilers, is that in which both the elongation and the reduction of area are high; say, from 28 per cent. to 30 per cent. elongation, and from 56 per cent. to 60 per cent. reduction of area. The reduction of area should be, and generally is, in an 8-inch section, just double the elongation. When they are both low, the tensile strength is apt to be high, and the quality inferior.

It is to be regretted that we could not anneal the test-pieces without annealing the plates, as this would have removed all variations in the results, due to differences of mechanical treatment, and perhaps would have prevented anomalous results. So much has been written on the treatment of steel, that it would be out of place for me in this paper, to do more than mention in a general way that many of the failures which manufacturers experience with steel plates, are due to the improper manner in which they handle them. Moreover, it is certain that steel plates, after having been flanged and punched, ought to be annealed; for even if they do stand without cracking the severe handling which they receive, they go into the boiler in a state of strain or tension that is dangerous.

The 5th test of No. 464 has been inserted to show what different results can be obtained with the same metal under different treatment. This test was taken from a plate made for the centre-keelson of the *Dolphin*. It was a difficult plate to make, being about 40 feet long,

5 feet wide, and nearly half an inch thick, and was finished at a black heat, giving results as shown in the table. When, however, a test-piece from the same plate was annealed, the trouble was immediately remedied, and the test gave 64,000 tensile strength and over 25 per cent. elongation.

Finally, I wish to call the attention of the Institute to what I consider the most important point developed by these tests, viz., the necessity of having some uniformity in the methods of making physical tests. When we read of a piece of metal having a tensile strength of 60,000 lbs., and a ductility of 25 per cent., we should know just what these figures mean. With the present methods of testing, they may mean much or little. Let standard test-pieces be established and specifications be made, based on such standards. Moreover, unless the conditions under which the tests are made are precisely uniform, no two tests can be compared with each other; for not only do the size and shape of test-pieces affect the results, but also the time, manner of applying the loads, duration of stresses, etc., etc. I give below a few tests, showing the different results obtained under different conditions.

The first test shows the effect of different thicknesses of the same metal.

Dimensions.	Tensile Strength.	Elongation in 8 inches.
·762 x ·7 inches.	52,600 pounds.	30·8 per cent.
1·25 x ·36 "	57,200 "	29·68 "

In this case, by diminishing the thickness of the plate from ·7 to ·36 inches, we increase the tensile strength 4,600 lbs. These differences are still more marked and astonishing if we break test-pieces of different sections from 1 inch to 8 inches. Whether the loads be applied rapidly or slowly, also makes a difference.

Section.	Dimensions.	Tensile Strength.	Elongation.	
1 inch.	·624 x ·602 inch.	75,000 pounds.		across grain.
8 "	·8 x ·605 "	65,600 "	23·8 per cent.	
1 "	·633 x ·591 "	73,500 "		with grain.
8 "	·8 x ·588 "	65,800 "	22·8 per cent.	

The above tests were all carefully made on steel furnished for boiler-plate by a well-known manufacturer in this country. Again with our steel:

	Section.	Tensile Strength.	Red. of Area.	Elongation.
A.	1 inch.	67,300 pounds.	58 per cent.	34 per cent.
A.	8 "	58,000 "	65 per cent.	25 per cent.
B.	1 "	69,700 "	46 per cent.	31 per cent.
B.	8 "	57,000 "	58 per cent.	27 per cent.

Here we have a difference in the case of B of nearly 13,000 lbs. Besides the difference in section, however, the pieces of 1 inch section were all pulled fast; that is to say, no appreciable interval was allowed between the application of the loads as in the 8-inch sections. It would be easy to multiply such examples; but I think it must be evident from the above that the consumer might as well not make any specifications, unless he prescribes all the conditions of testing, viz., thickness of plate, size of test-piece, shape of test-piece, amount of load, duration of stresses, etc. In view of these facts, and the time, trouble, and expense of making so many physical tests, I believe the day is not far distant when chemical specifications alone will be prescribed. Indeed, it is quite possible now to recognize good or bad steel by its chemical analysis: and if some one will only give us a method for determining oxide of iron, it could be done with certainty every time. Chemical methods are becoming more rapid and accurate every day. Combined carbon can now be determined in five minutes and manganese in one hour, and with a previous knowledge of the stock from which the steel is made, there is little more to be desired.

It gives me great pleasure to acknowledge the valuable services of my assistants, Mr. Josef Westesson, who made most of the chemical analyses, and Mr. Loudon Richards, who had charge of the physical tests.

Heat No.	Dimensions of test piece.	Area.	Area of least section after breaking.	Original length of minimum section measured between shoulders.	Length after breaking.	Breaking strain.	Tensile strength per square inch.	Reduction of cross-section or area.	Ultimate elongation in 8 in.	Carbon before addition of ferro-manganese.	Carbon.	Phosphorus.	Manganese.
	Inches.	Sq. in.	Sq. in.	Inches.	In.	lbs.	lbs.	p. ct.	p. ct.				
453	1.125 x .35	.3937	.12	8	10.186	20,870	53,010	69	27.4		.10	.036	.17
464	1.12 x .59	.6628	.2962	8	10.156	43,350	65,405	55.3	26.95		.19	.039	.37
464	.936 x .703	.6580	.3049	8	10.19	43,500	66,100	53.6	27.38				
464	.985 x .695	.6846	.3115	8	10.09	47,200	68,940	54.5	26.12				
464	.967 x .715	.6914	.2934	8	10.017	45,250	65,300	57.7	25.21				
464	1.24 x .455	.5642	.3056	8	9.58	40,000	70,900	45.8	19.7	rol	led	cold.	
465	1.245 x .482	.60	.2511	8	10.254	36,600	61,000	58.	28.2				
*465	1.25 x .482	.6025	.3199	8	9.978	36,000	59,580	53.	24.74		.13	.035	.38
466	.96 x .586	.5625	.2935	8	9.934	41,300	73,300	47.8	24.2		.20	.030	.37
*466	1.127 x .442	.5149	.27	8	9.63	36,000	69,916	57.2	20.37				
467	.928 x .57	.529	.2412	8	9.895	36,390	68,790	54.	23.7				
*467	.984 x .579	.5697	.3125	8	10.206	39,330	69,000	47.	27.82				
468	1.02 x .285	.2907	.1417	8	9.675	19,160	65,910	48.	20.93				
469	.975 x .448	.4368	.196	8	10.12	27,500	62,900	55.	26.5				
*469	1.264 x .6	.7584	.3656	8	10.026	46,500	61,313	51.7	25.32				
470	.921 x .569	.5240	.2501	8	10.383	34,100	65,000	52.	29.8				
*470	.989 x .575	.5686	.2956	8	9.9	36,400	64,000	50.	23.7				
471	1.01 x .454	.4585	.1891	8	10.161	26,000	56,600	58.7	27.01				
*471	1.25 x .595	.7437	.3471	8	9.875	46,540	62,600	53.	23.44				
472	1. x .46	.46	.1925	8	10.17	27,700	60,130	58.	26.13				
*472	1. x .463	.463	.232	8	9.942	27,600	59,600	50.	24.28				
473	1.3 x .438	.5694	.2571	8	10.25	33,950	59,600	54.8	28.13				
*473	1.035 x .55	.5692	.2142	8	10.262	33,700	59,200	62.5	28.15				
474	1.155 x .576	.6653	.2508	8	10.469	36,800	55,300	62.	30.86				
475	.985 x .488	.4806	.1965	8	10.04	27,700	57,630	59.1	30.				
*475	.985 x .484	.4767	.1885	8	10.32	27,550	57,790	56.2	29.				
476	.94 x .5	.47	.165	8	10.255	24,900	53,000	64.8	28.25				
477	1.235 x .441	.5446	.261	8	10.29	31,900	58,500	52.1	28.63				
*477	1.261 x .441	.5561	.2594	8	10.31	32,150	57,800	53.1	28.88				
478	1. x .615	.615	.255	8	10.408	34,200	55,600	58.5	30.1				
479	.999 x .529	.5284	.206	8	10.138	29,300	55,400	61.	26.98				
480	1.028 x .56	.5756	.261	8	10.375	34,000	59,000	56.3	29.68				
*480	1. x .564	.564	.2968	8	10.289	32,400	57,400	47.3	28.61				
481	1. x .475	.475	.2131	8	10.244	26,300	55,790	55.	28.05		.11		.36
482	1.266 x .495	.6266	.3035	8	10.143	40,000	63,836	51.5	26.79	.18	.20		.38
*482	1.266 x .504	.6380	.3633	8	9.68	41,000	64,500	43.	21.				
483	1.225 x .485	.5941	.2455	8	10.208	32,800	55,200	58.7	27.60	.09	.11		.36
484	1.205 x .418	.5036	.2728	8	9.882	35,800	71,000	45.8	23.52	.15	.21		.40
*484	1.254 x .44	.5517	.3451	8	9.688	39,100	79,860	37.4	21.1				
485	1.25 x .507	.6337	.252	8	10.272	35,000	55,200	60.2	28.4	.09	.11		.34
486	1.135 x .50	.5675	.2730	8	10.275	36,130	63,665	51.8	28.4	.10	.12		.37
*486	1.24 x .513	.6361	.3648	8	9.78	39,100	61,400	42.6	22.25				
487	1.19 x .575	.6842	.2788	8	10.17	41,150	60,143	59.25	27 25	.14	.15		.37
*487	1.249 x .586	.7219	.3356	8	10.084	44,000	60,950	53.5	26.05				
488	1.25 x .525	6562	.264	8	10.545	36,800	56,080	59.9	31.81	.10	.14		.39
489	1.072 x .472	.5059	.2385	8	9.875	30,000	59,300	52.8	24.68	.09	.13		.40
*489	1.216 x .481	.5993	.2557	8	10.025	35,300	59,280	57.9	25.31				
490	1.187 x .339	.4023	.1842	8	9.66	29,600	73,500	54.2	20.75	.16	.21		.56
491	1.264 x .408	.514	.2053	8	10.016	32,900	64,000	60.	25.2	.13	.17		.50
*491	1.262 x .411	.5186	.2491	8	10.046	32,700	63,000	51.9	25.59				
492	1.256 x .292	.3677	.1516	8	10.	23,650	64,310	57.	25.	.09	.13		.49
*492	1.265 x .293	.3706	.1780	8	10.264	23,650	63,860	51.9	28.3				
493	1.26 x .509	.6413	.3521	8	9.983	44,000	68,600	45.	24.78	.14	.16		.53
*493	1.254 x .361	.4527	.2055	8	10.025	30,000	66,200	54.6	25.31				
494	1.217 x .39	.4746	.2187	8	9.959	31,300	65,900	53.9	24.48	.10	.16		.50
*494	1.25 x .387	.4837	.2269	8	10.029	31,900	65,900	53.	25.36				
495	1.25 x .342	.4275	.1938	8	9.970	26,300	61,500	54.7	24.62	.09	.13	.062	.49
*495	1.25 x .348	.4351	.2229	8	10.045	26,000	59,700	46.4	25.6				
496	1.25 x .275	.3437	.1828	8	9.435	23,460	67,660	46.8	17.87	.11	.15	.046	.50
497	1.254 x .296	.3701	.1702	8	9.96	23,300	62,900	54.	21.5	.09	.13		.46
*497	1.25 x .315	.3937	.1896	8	9.854	25,300	64,700	49.3	23.1				
498	1. x .47	.47	.2142	8	9.912	30,000	64,000	54.4	23.9	.12	.16	.058	.49
498	1. x .475	.475	.2338	8	10.025	31,800	66,900	50.8	25.2				
499	1.25 x .285	.3562		8	10.	23,950	67,300	52.3	25.	.09	.15	.031	.50
499	1.25 x .298	.3725		8	9.945	25,300	67,900	53.4	24.31				
500	1. x .5	.5		8	10.256	31,500	63,100	55.2	28.2	.11	.15		.49
500	1. x .5	.5		8	10.144	31,600	63,200	55.3	26.8				
501	1.23 x .327	.4022		8	9.98	26,290	66,630	47.4	24.75	.11	.14		.56
501	1.2 x .321	.3852		8	10.128	24,900	64,640	50.	26.6				
502	1.237 x .37	.4576		8	10.048	28,450	62,200	52.3	25.6	.11	.15		.50
502	1.237 x .37	.4576		8	10.048	29,100	63,500	52.	25.6				

Heat No.	Dimensions of test piece.	Area	Area of least section after breaking.	Original length of minimum section measured between shoulders.	Length after breaking.	Breaking strain.	Tensile strength per square inch.	Reduction of cross-section or area.	Ultimate elongation in 8 in.	Carbon before addition of ferro-manganese.	Carbon.	Phosphorus.	Manganese.
	Inches.	Sq.in.	Sq.in.	Inches.	In.	lbs.	lbs.	p. ct.	p. ct.				
503	1.25 x .432	.5400	.2991	8	10.008	39,700	73,600	44.5	25.1	.15	.17		.58
503	1. x .49	.49	.2599	8	9.955	34,450	70,300	49.	24.44				
503	.98 x .455	.4459	.2050	8	9.9	31,700	71,000	54.	23.75				
503	1. x .455	.455	.2117	8	9.864	32,700	71,860	53.4	23.3				
503	1.25 x .43	.5375	.2949	8	9.861	39,600	73,600	45.1	23.26				
504	.992 x .484	.4801	.2304	8	10.112	30,800	64,100	52.	26.4	.09	.17		.45
504	1.007 x .484	.4873		8	9.875	31,350	64,300	52.9	23.43				
505	1. x .453	.453	.1845	8	10.333	28,000	61,800	59.2	29.1	.09	.15		.39
505	.97 x .462	.4481	.2016	8	10.085	27,500	61,600	55.	26.06				
506	.975 x .485	.4728	.1961	8	10.164	29,700	62,800	58.5	27.	.08	.14		.43
506	.975 x .485	.4728	.1902	8	10.100	29,200	61,750	59.7	26.31				
507	1.167 x .433	.5053	.2485	8	9.911	33,000	65,300	50.8	23.88	.10	.15		.54
507	1.242 x .425	.5278	.2682	3	10.06	35,000	66,310	49.2	25.83				
508	1.25 x .472	.59	.3058	8	10.225	37,000	62,700	48.1	27.8	.11	.14		.49
508	1.25 x .478	.5975	.2765	8	10.062	37,400	62,400	53.7	25.77				
509	1.25 x .46	.575	.261	8	10.275	35,400	61,560	54.6	28.43	.08	.12		.53
509	1.25 x .456	.57	.2781	8	9.937	36,000	63,000	51.2	24.21				
510	1.25 x .463	.5787	.2579	8	9.965	35,700	61,960	55.4	24.6	.08	.15		.47
510	1.245 x .465	.5785	.2566	8	10.165	36,000	62,100	55.6	27.6				
511	1. x .467	.467	.2277	8	9.97	28,800	61,600	51.2	24.63	.07	.15		.48
511	1. x .478	.478	.2156	8	10.284	29,000	60,700	54.9	28.55				
512	1. x .485	.485	.2205	8	10.08	31,300	64,530	54.5	26.	.13	.16		.48
512	1. x .489	.489	.228	8	9.848	31,500	64,400	53.1	23.1				
513	.99 x .464	.4593	.2044	8	10.120	28,450	61,000	55.4	26.5	.09	.14		.47
513	.982 x .46	.4517	.1995	8	10.075	27,500	60,800	55.8	25.9				
514	.983 x .45	.4423	.1883	8	9.796	27,800	62,800	57.8	22.45	.12	.15		.40
514	.983 x .435	.4276	.1969	8	9.960	27,150	63,900	53.9	24.5				
515	1.242 x .385	.4781	.2405	8	9.975	32,600	68,100	49.6	24.7	.12	.16		.53
515	1.23 x .38	.4674	.2481	8	9.975	31,900	68,200	47.	24.7				
516	1.233 x .48	.5918	.2555	8	10.115	37,400	63,000	56.8	26.43	.10	.16		.43
516	1.225 x .475	.5818	.258	8	10.145	37,200	63,900	55.5	26.81				
517	1.25 x .455	.5687	.2634	8	9.927	37,400	65,750	53.6	24.08	.17	.22		.43
517	1.243 x .441	.5481	.2714	8	9.872	36,400	66,400	50.4	23.4				
518	1.254 x .404	.5066	.2312	8	10.	34,000	67,100	58.7	25.	.15	.19		.43
518	1.25 x .404	.505	.2320	8	10.	33,900	67,100	54.	25.				
519	.964 x .44	.4242	.21	8	10.133	27,200	64,100	50.5	26.6	.13	.18		.49
519	.972 x .435	.4228	.198	8	9.892	27,850	65,800	52.9	23.67				
520	.969 x .69	.6685	.3368	8	9.885	41,850	62,600	49.4	23.6	.12	.15		.46
520	.985 x .685	.6747	.3334	8	10.209	41,600	61,600	49.8	27.6				
521	.962 x .722	.6945	.3307	8	10.16	44,250	63,700	52.3	27.	.11	.14		.44
521	.988 x .460	.4545	.1918	8	10.17	29,500	64,000	57.8	27.1				
522	.981 x .686	.6730	.2903	8	9.986	43,900	65,200	55.3	24.8	.12	.16		.46
522	.985 x .688	.6775	.3588	8	9.82	46,200	68,200	47.	22.75				
523	.995 x .707	.7034	.2979	8	10.116	43,750	62,200	57.6	26.45	.09	.14		.38
523	.997 x .711	.7088	.3106	8	10.272	43,280	61,080	54.6	28.4				
524	.985 x .71	.6993	.3471	8	10.	42,900	61,300	50.3	26.25	.10	.14		.39
524	.988 x .71	.7015	.3118	8	10.161	42,800	61,000	55.5	27.01				
525	1.24 x .531	.6584		8	9.718	44,800	68,000		21.47	.12	.16		.46
525	1.25 x .527	.6587	.3672	8	9.762	42,800	64,900	44.3	22.02				
526	.967 x .705	.6817	.3299	8	9.84	41,000	60,120	51.6	23.	.08	.14		.46
526	.963 x .698	.6721	.3230	8	10.117	40,750	60,620	51.9	26.46				
527	.967 x .706	.6827	.3503	8	9.995	43,650	63,930	48.6	24.93	.12	.14		.41
527	.985 x .705	.6944	.3431	8	9.855	43,500	62,640	50.5	23.18				
528	.997 x .708	.7059	.3501	8	10.063	52,500	74,370	50.4	25.78	.15	.21		.46
528	.989 x .709	.7012	.36	8	9.825	52,400	74,700	48.6	22.81				
529	.983 x .700	.6881	.3272	8	10.018	43,000	62,480	51.	25.22	.10	.16		.40
529	1. x .695	.695	.3371	8	10.085	43,350	62,370	51.5	26.06				
530	.985 x .703	.6924	.343	8	9.9	48,000	69,300	50.	23.75	.18	.18		.43
530	.98 x .700	.686	.3127	8	9.8	48,100	70,000	54.4	22.5				
531	.995 x .575	.5721	.3013	8	9.875	38,500	67,290	47.3	23.43	.12	.16		.35
531	.998 x .577	.5728	.2815	8	10.136	37,000	64,200	51.1	26.7				
532	.978 x .554	.5418	.2874	8	9.875	38,950	71,900	46.9	23.43	.13	.17		.35
532	.985 x .555	.5466	.2893	8	9.785	37,850	69,240	48.5	23.37				
533	.975 x .832	.7912	.3630	8	10.10	47,700	60,260	54.	26.25	.11	.14		.30
533	.983 x .837	.8227	.3169	8	10.38	47,200	57,370	61.5	29.75				
534	.975 x .715	.6971	.2756	8	10.22	40,000	57,370	60.4	27.75	.08	.12		.25
534	.974 x .719	.7003	.2893	8	10.323	40,050	57,180	58.6	29.04				
535	.963 x .712	.6856	.2[illegible]50	8	10.225	39,000	56,900	62.2	27.81				
536	.95 x .693	.6583	.2634	8	10.333	35,850	54,450	56.5	29.16	.06	.11		.45
536	.865 x .70	.6055	.2408	8	10.25	32,550	53,750	58.5	28.12				
537	1.234 x .375	.4627	.1898	8	10.127	27,900	60,3[illegible]0	58.9	26.83	.09	.14		.36

Heat No.	Dimensions of test piece.	Area.	Area of least section after breaking.	Original length of minimum section measured between shoulders.	Length after breaking.	Breaking strain.	Tensile strength per square inch.	Reduction of cross-section or area.	Ultimate elongation in 8 in.	Carbon before addition of ferro-manganese.	Carbon.	Phosphorus.	Manganese.
	Inches.	Sq.in.	Sq.in.	Inches.	In.	lbs.	lbs.	p. ct.	p. ct.				
537	1.262 x .368	.4644	.2061	8	10.145	28,100	60,720	55.6	26.81				
538	1.277 x .498	.6359	.2378	8	10.5	35,900	56,400	62.6	31.25	.05	.11		.24
538	1.035 x .492	.5092	.1950	8	10.291	28,600	56,100	61.7	28.63				
539	1.216 x .492	.5982	.2407	8	10.2	33,600	56,160	58.	27.5	.07	.14		.33
539	1 036 x .480	.4972	.2044	8	10.10	29,350	59,030	58.8	26.2				
539	1.038 x .475	.4930	.2027	8	10.175	29,260	59,300	58.9	27.4				
540	1.25 x .36	.45	.225	8	10.085	28,000	62,200	50.	25.81	.05	.10		.31
540	1.312 x .356	.4671	.2339	8	9.875	28,850	61,900	49 9	23.43				
541	1.210 x .344	.4162	.1877	8	9.96	25,200	60,250	54.9	24.5	.04	.13		.29
541	1.25 x .345	.4312	.1870	8	10.05	26,000	60,296	56.6	25.62				
542	1.210 x .375	.1537	.2231	8	10.15	27,450	60,500	50.8	26.87	.05	.14		.36
542	1.25 x .370	.4625	.1991	8	10.332	27,900	60,320	50.5	29.15				
543	1.205 x .410	.4940	.1961	8	10.457	28,260	57,200	60.	30.7	,05	.15		.31
543	1.255 x .416	.5220	.2142	8	10.30	29,850	57,180	58.9	28.75				
544	1.210 x .463	.5602	.2422	8	10.40	30,800	54,800	56.7	30.	.06	.14		.27
545	1.261 x .335	.4224	.1833	8	10.267	23,100	54,600	56.	28.33	.06	.13	.061	.32
546	1.271 x .398	.5058	.2129	8	10.187	29,500	58,300	57.9	27.3	.09	.14	.057	.29
546	1.263 x .4	.5052	2378	8	10.172	29,500	58,300	52.9	27.15				
547	1.256 x .432	.5425	.2111	8	10.512	31,000	57,100	61.	31.4	.06	.15	.058	.30
547	1.262 x .428	.5401	.2013	8	10.425	31,200	57,800	62.	30.3				
548	1.258 x .35	.4403	.1871	8	10.405	25,800	58,600	57.5	30.	.10	.16	.069	.27
548	1.243 x .357	.4437	.1744	8	10.288	26,500	59,700	60.6	28.6				
549	1.204 x .445	.5257	.2320	8	10 20	33,300	63,300	55.8	27.5	.08	.19	.069	.34
549	1.032 x .443	.4571	.1952	8	10.23	28,950	63,300	57.2	27.93				
550	1.260 x .325	.4095	.1952	8	10.245	26,700	65,200	50.	28.06	.05	.15	.064	.40
550	1.255 x .34	.4267	.1913	8	10.310	26,700	62,570	55.	28.87				
551	1.250 x .432	.5400	.2119	8	10.50	29,650	54,900	60.7	31 25	.04	.11	.075	.28
552	1.148 x .415	.4764	.1881	8	10.358	27,550	57,820	56.3	29.47	.04	.13	.075	.30
552	1.165 x .410	.4776	.2093	8	10.225	27,550	57,680	56.2	27.8				
553	1.160 x 370	.4292	.1581	8	10.45	22,400	52,190	62.6	30.6	.04	.11	.051	.25
553	1.115 x .373	.4158	.1548	8	10.36	21,700	52,180	62.8	29.5				
554	1.125 x .415	.4668	.2131	8	10.29	28,100	60,200	54.3	28.6	.06	.16	.062	.42
554	.995 x .417	.4149	.1709	8	10.208	25,150	60,600	58.7	27.6				
555	1.064 x .419	.4458	.1966	8	10.325	26,200	58,800	55.9	29.06	.04	.14	.064	.28
555	1.115 x .392	.4370	.1860	8	10.265	25,900	59,200	57.2	28.2				
556	.750 x .672	.5040	.2371	8	10.385	30,150	59,800	52.9	29.8	.06	.15	.043	.30
556	.773 x .71	.5488	.2202	8	10.275	31,500	57,400	59.8	28 43				
557	.762 x .7	.5334	.2227	8	10.465	28,100	52,600	52.	30 8	.06	.14	.050	.29
557	1.25 x .36	.45	.2117	8	10.375	25,750	57,220	53.7	29.68				
557	1.312 x .37	.4854	.1980	8	10.06	27,650	56,960	57.	25.75				
558	1.25 x .375	.4688	.2612	8	9.875	30,600	65,200	44.3	23.43	.06	.15	.046	.38
558	1.26 x .385	.4851	.2327	8	9.915	29,700	61,000	52.	23.9				
559	1.03 x .346	.3563	.1435	8	10.36	20,500	57,500	59.7	29.5	.07	.16	.052	.27
559	1.015 x .347	.3522	.1396	8	10.25	20,500	28,200	60.	28.1				
560	1.023 x .324	.3314	.1384	8	10.325	20,500	56,400	58.5	29 06	.06	.11	.052	.30
561	1.035 x .485	.5019	.2495	8	10.03	30,500	60,760	50.	25.37	.08	.16	.039	.39
561	1.025 x .482	.494	.2283	8	10.15	29,750	60,220	53.9	26.87				
562	.975 x .47	.4582	.1881	8	10.25	26,400	57,600	58.9	28.10	.09	.16	.050	.28
562	1.005 x .475	.4773	.2006	8	10.40	27,650	57,900	58.	30.				
563	.988 x .462	.4564	.1859	8	10.375	26,150	57,300	59.	29 7	.08	.14	.044	.33
563	1. x .46	.46	.1965	8	10.31	26,300	58,000	57.3	28.9				
564	1.310 x .348	.4559	.2005	8	10.066	26,850	59,100	56.	25 8	.08	.16	.037	.40
564	1.312 x .353	.4631	,1778	8	9.975	26,800	57,900	61.1	24.7				
565	.975 x .538	.5245	.2244	8	10.197	31,350	59,790	57.2	27 46	.12	.18	.038	39
565	1.005 x .54	.5427	.2401	8	10.343	33,300	61,300	55.7	29.3				
566	.985 x .53	.522	.2145	8	10.219	30,500	58,430	58.9	27.73	.11	.20	.039	.41
566	.975 x .525	.5118	.2168	8	10 468	30,500	59,600	57.4	30.8				
567	1.017 x .53	.539	.2167	8	10.25	31,450	58,350	60.9	28.10	.09	.17	.05	.27
567	1.025 x .537	.5504	.2146	8	10.468	31,600	57,400	61.	30 8				
568	1.028 x .5	.5140	.2289	8	10.25	32,150	63,100	55.4	28.1	.11	.17	.044	.42
568	1.023 x .495	.5064	.2376	8	10.188	30,750	60,700	53.	27.34				
569	1.027 x .368	.3779	.2123	8	9.969	24,900	65,800	41.	24.6	.11	.17	.042	.44
569	1.025 x .350	.3587	.1937	8	9.719	23,900	66,600	46.	21.485				
570	1.016 x .380	.3859	.1704	8	10.375	24,700	64,500	55.8	29.68	.11	.17		.42
570	1.022 x .365	.3730	.1810	8	10.125	23.600	63,200	51.4	26.56				
571	1.028 x .594	.6111	.3433	8	9.72	38.200	62,500	43.8	21.5	.11	.16	.047	.41
571	1.028 x .595	.6116	.3257	8	10.09	38,700	63,300	46.7	26.175				
572	1.034 x .565	.5842	.2772	8	10.47	33,500	57,340	52.5	30.86	.10	.15	.053	.37
572	1.03 x 565	.5819	.2645	8	10.28	34,100	57,800	54.	28.5				
573	1.035 x .7	.7245	.4517	8	10.06	44,400	61,200	37.	25.78	.09	.17	.039	.29
573	1.048 x .7	.7335	.3253	8	10.22	42,250	57,600	55 6	27.1				

Heat No.	Dimensions of test piece.	Area.	Area of least section after breaking.	Original length of minimum section measured between shoulders.	Length after breaking.	Breaking strain.	Tensile strength per square inch.	Reduction of cross-section or area.	Ultimate elongation in 8 in.	Carbon before addition of ferro-manganese.	Carbon.	Phosphorus.	Manganese.
	Inches.	Sq in.	Sq.in.	Inches.	In.	lbs.	lbs.	p. ct.	p. ct.				
571	1.023 x .69	.7058	.3045	8	10.312	41,100	58,200	56.8	28.9	.08	.14	.038	.35
571	1.05 x .712	.7476	.2938	8	10.281	41,900	56,000	60.	28.5				
675	.815 x .572	.4661	.1981	8	10.064	28,000	60,000	57.3	25.8	.07	.13	.042	.30
575	.810 x .565	.4746	.1799	8	10.187	27,850	58,700	61.	27.3				
576	.99 x .565	.5594	.2292	8	10.313	34,500	61,700	59.	28.9	.08	.14	.018	.36
576	.975 x .565	.5509	.2368	8	10.	33,600	60,900	58.8	25.				
577	.96 x .56	.5280	.2063	8	10.313	50,300	57,400	60.	28.9	.08	.14	.033	.39
577	.98 x .55	.539	.2010	8	10.56	30,550	56,700	62.	32.				
578	.98 x .552	.5409	.2112	8	10.56	31,300	57,860	60.9	32.		.14	.050	.40
578	.851 x .555	.4739	1776	8	10.28	27,450	57,900	62.	28.5				
579	.788 x .548	.4318	.1806	8	10.187	25,700	59,500	58.1	27.3		.15	.047	.37
579	.8 x .545	.4360	.1839	8	10.25	26,500	60,700	59.	28.1				
580	1.098 x .71	.7157	.2992	8	10.031	43,000	60,000	56.8	25.31	.12	.17	.042	.41
580	.755 x .709	.5353	.2175	8	9.97	32,350	60,430	59.3	24.63				
581	1.065 x .706	.7518	.3249	8	10.534	43,100	57,300	56.6	31.64	.07	.14	.047	.33
581	1.154 x .71	.8193	.3449	8	10.25	48,250	59,800	57.9	28.1				
582	1.115 x .575	.6411	.3097	8	10.56	37,150	57,900	51.6	32.	.08	.15	.048	.35
582	1.08 x .57	.6156	.2888	8	10.375	35,600	57,800	51.4	29.68				
583	1.036 x .546	.5556	.2241	8	10.188	31,850	57,300	60.	27.34	.07	.15	.055	.32
583	1.068 x .543	.5799	.2185	8	10.591	32,400	55,910	57.1	32.42				
585	1.252 x .468	.5859	.2651	8	10.316	38,000	64,800	54.5	28.9				
585	1.252 x .465	.5831	.2693	8	9.9	39,300	67,300	53.8	23.7				
586	1.25 x .475	.5937	.2889	8	10.05	35,100	59,120	51.3	25.62	.08	.13	.052	.33
586	1.245 x .47	.5851	.2575	8	20.127	34,800	58,100	55.9	26.6				
587	1.218 x .475	.5932	.2562	8	10.31	31,650	58,400	56.8	28.9	.08	.14	.046	.33
587	1.24 x .47	.5828	.3138	8	10.152	37,000	63,400	41.2	26.9				
588	1.212 x .495	.6148	.225	8	10.072	38,700	62,900	60.	25.9	.12	.16	.018	.43
588	1.252 x .503	.6297	.2912	8	9.953	38,000	60,300	53.7	24.4				
589	1.31 x .475	.6222	.3021	8	9.91	39,900	61.400	51.	23.9	.12	.16	.016	.49
589	1.3 x .471	.6123	.2895	8	10.05	40,000	65,300	52.7	25.6				
590	1.228 x .48	.5894	.2676	8	10.121	36,150	61,300	54.6	26.5	.08	.14	.052	.39
590	1.255 x .488	.6027	.3078	8	9.904	35,750	59,300	48.9	23.8				
591	1.225 x .488	.5978	.3063	8	10.065	39,150	65,900	48.9	25.81			.059	.46
591	1.258 x .49	.6161	.3114	8	10.088	39,150	63,400	49.4	26.1				
592	1.236 x .518	.6102	.3438	8	10.05	42,150	65,800	46.3	25.6	.12	.14	.045	.41
592	1.22 x .519	.6332	.3881	8	10.	41,400	65,380	38.6	25.				
593	1.28 x .505	.6464	.3421	8	10.104	41,150	63,600	47.	26.3	.10	.19	.054	.44
593	1.279 x .503	.6433	.3139	8	10.	39,500	61,400	50.	25.				
594	1.271 x .494	.6194	.3204	8	10.109	38,150	61,500	48.2	26.36	.09	.15	.052	.42
594	1.275 x .490	.6247	.3067	8	10.128	39,750	63,600	50.	26.6				
595	1.26 x .448	.5644	.2441	8	10.25	52,750	57,800	56.7	28.1	.08	.13	.046	.36
595	1.26 x .415	.5607	.2651	8	10.110	53,500	59,700	52.7	26.4				
596	1.215 x .487	.5917	.3122	8	9.97	36,650	61,900	40.7	21.6	.12	.16	.037	.42
596	1.19 x .480	.5722	.3045	8	10.067	37,250	65,000	46.	25.8				
597	1.22 x .471	.5746	.2961	8	9.915	37,500	65,200	48.4	23.93	.12	.17	.056	.36
597	1.225 x .472	.5782	.2816	8	9.878	36,900	63,800	51.	23.5				
598	1.227 x .505	.6196	.2608	8	10.158	36,100	58,200	57.8	26.9	.07	.12	.051	.31
598	1.21 x .498	.6175	.2621	8	10.275	33,950	54,900	57.5	28.4				
600	1.254 x .502	.6295	.3096	8	9.76	41,250	65,300	50.8	22.	.12	.18	.060	.40
600	1.23 x .501	.6162	.3255	8	9.775	41,100	66,600	47.	22.2				
601	1.222 x .485	.5926	.3195	8	10.	38,200	64,400	46.	25.	.13	.18	.048	.37
601	1.225 x .48	.588	.3147	8	10.045	38,300	65,100	46.4	25.5				
603	1.252 x .492	.6169	.3895	8	9.66	43,200	70,000	37.	20.8		.19	.056	.73
603	1.26 x .497	.6262	.3791	8	9.775	43,500	69,400	39.3	22.2				
605	1.12 x .44	.4928	.3066	8	9.89	31,400	63,700	38.	23.6	.11	.17	.044	.50
605	1.25 x .438	.5475	.3240	8	10.92	35,900	65,500	40.8	25.3				
606	1.175 x .509	.598	.3589	8	9.575	38,900	65,000	40.	19.7	.09	.14	.042	.47
607	.769 x .706	.5429	.2971	8	9.92	36,150	66,500	45.2	21.	.09	.15	.018	.29
607	1.215 x .704	.8765	.3595	8	10.075	55,200	63,000	47.4	25.7				
608	1.217 x .512	.6231	.2985	8	10.033	36,700	58,900	52.	25.4		.16	.043	.27
608	1.2 x .506	.6072	.2882	8	10	37,250	61,200	52.	25.				
609	1.21 x .509	.6159	.3311	8	9.875	40,250	65,000	46.2	23.43	.09	.15	.052	.29
610	1.195 x .473	.5652	.2720	8	10.375	37,300	65,900	51.8	29.67	.08	.15	.062	.38
610	1.243 x .476	.5917	.303	8	9.96	37,800	64,000	51.2	21.5				
611	1.179 x .473	.5577	.3027	8	9.841	36,100	64,700	45.7	23.01	.09	.17	.047	.38
611	1.245 x .469	.5839	.3096	8	10.25	38,100	65,000	47.	28.1				
612	1.231 x .486	.5983	.3276	8	9.938	37,000	61,800	45.2	21.22		.16	.065	.32
612	1.249 x .481	.6045	.2855	8	9.855	39,500	54,900	52.7	23.2				
613	1.245 x .455	.5665	.3007	8	10.	35,250	62,200	47.	25.	.09	.16	.061	.39
613	1.244 x .458	.5697	.3092	8	9.8	36,300	63,700	44.	22.5				
614	1.238 x .46	.5695	.2866	8	10.312	35,000	61,100	49.6	25.39	.08	.16	.051	.39

Heat No.	Dimensions of test piece.	Area.	Area of least section after breaking.	Original length of minimum section measured between shoulders.	Length after breaking.	Breaking strain.	Tensile strength per square inch.	Reduction of cross-section or area.	Ultimate elongation in 8 in.	Carbon before addition of ferro-manganese.	Carbon.	Phosphorus.	Manganese.
	Inches.	Sq.in.	Sq in.	Inches.	In.	lbs.	lbs.	p. ct.	p. ct.				
614	1.242 x .458	.5688	.2946	8	10.38	34,500	60,600	48.2	29.7				
615	1.24 x .47	.5828	.2861	8	10.185	36,100	61,900	50.9	27.3	.10	.17	.051	.41
615	1.248 x .473	.5903	.3034	8	10.	37,100	62,800	48.6	25.				
616	1.257 x .5	.6285	.2882	8	10.317	38,750	61,600	54.	28.9	.09	.16	.057	.41
616	1.271 x .5	.6355	3094	8	9.945	30,750	62,500	51.	24.3				
617	1.255 x .51	.64	.3034	8	10.06	43,400	67,800	47.4	25.75	.10	.20	.066	.39
617	1.270 x .512	.6502	.3492	8	10.06	43,650	67,100	46.3	25.75				
618	1.27 x .513	.6515	.3718	8	9.84	41,850	64,000	42.9	23.		.15	.057	.45
618	1 25 x .513	.6412	3864	8	9.88	45,000	70,000	39.7	23.5				
619	1.26 x .548	.6905	.380	8	10.225	43,850	63,500	42.	27.8		.16	.059	.36
619	1.266 x .552	.6988	.4610	8	9.955	43,750	62,600	34.	24.3				
620	1.252 x .497	6222	.3208	8	9.848	44,900	72,300	48.4	23.1				
620	1.255 x .5	.6275	.3686	8	9.880	46,500	74,000	41.	23.5				
621	1.248 x .545	.6802	.3285	8	10.025	42,400	62,300	51.8	25.3		.15	.048	.36
621	1.245 x .54	.6723	.3321	8	9.955	42,500	63,200	50.6	24.4				
622	1.25 x .539	.6737	.3474	8	10.085	42,500	63,000	49.3	26.06		.17	.051	.47
622	1.25 x .540	.675	.3494	8	10.166	43,000	63,700	48.2	27.07				
623	1.244 x .48	.5971	.3188	8	10.103	38,000	63,600	46.6	26.29		.15	.060	.43
623	1.248 x .487	.6077	.3168	8	10.178	38,600	63,000	47.8	27.2				
624	1.248 x .487	.6078	.3226	8	10.150	41,000	67,400	46.9	26.87		.15	.052	.41
624	1.25 x .485	.6062	.3077	8	10.252	42,000	69,000	49.	28.				
625	1.25 x .465	.5812	.2835	8	10.08	38,750	66,200	51.	26.		.16	.061	.38
625	1.27 x .468	.5946	.3178	8	10.02	39,500	66,400	46.5	25.2				
626	1.245 x .465	.5789	.3042	8	9.905	40,300	69,400	47.4	23.8		.19	.045	.33
626	1.26 x .466	.5871	.3174	8	9.92	39,500	67,300	45.9	24.				
627	1.25 x .51	.6375	.3707	8	9.915	45,600	71,500	41.8	33.93		.19	.050	.41
627	1.25 x .512	.640	.36	8	10.12	46,000	71,900	45.3	26.5				
628	1.25 x .498	.6225	.2949	8	10.225	35,250	56,600	52.6	27.81		.12	.053	.36
628	1.25 x .490	.6125	.3063	8	10.	36,500	59,500	50.	25.				
630	1.43 x .525	.6526	.3456	8	9.85	39,250	60,100	47.	23.1		.13	.048	.49
630	.985 x .525	.517	.245	8	9.880	32,300	62,800	52.5	23.5				
631	1.195 x .491	.5867	.3204	8	9.885	37.500	63,900	45.3	23.56		.17	.051	.44
631	1.255 x .490	.6150	.36	8	9.868	38,900	63,200	41.4	23.35				
632	1.192 x .472	.5626	.2958	8	9.895	35,350	63,000	47.	23.7		.14	.052	.34
632	1.196 x .477	.5704	.2907	8	10.28	36,000	63,000	49.	28.5				
633	1 2 x .496	.5952	.2689	8	10.025	35,400	59,400	54.8	25.3		.15	.047	.41
633	1 214 x .494	.5997	.2774	8	10.085	34,700	57,800	53.7	26.				
634	1.209 x .5	.6045	.2992	8	10.06	37,250	61,600	50.5	25.7		.16	.051	.45
634	1.256 x .5	.628	.3240	8	9.95	38,750	61,700	48.4	24.3				
635	1 212 x .487	.5902	.2814	8	10.14	33,300	56,760	52.3	26.7		.13	.056	.36
635	1 2 x .488	.5856	.2797	8	10.055	33,800	57,700	52.2	25.7				
636	1 198 x .480	.5750	.2838	8	9.95	34,150	59,300	50.6	24.37		.15	.051	.39
636	1.2 x .485	.582	.3000	8	9.805	34,600	59,400	48.4	22.57				
637	1.198 x .482	.5774	.2520	8	10.24	30,900	53,500	56.3	28.		.13	.049	31
637	1.198 x .480	.5750	.2860	8	10.006	30,900	52,000	50.2	27.5				
638	1.252 x .53	.6636	.3591	8	9.925	43,600	65,700	45.8	24.06		.14	.049	.39
638	1.257 x .531	.6674	.4075	8	9.882	44,500	66,600	38.8	23.5				
639	1.215 x .523	.6511	.3139	8	10.05	37,300	57,280	51.7	25.6		.13	.05	.36
639	1.25 x .523	.6537	.3201	8	10.24	37,700	57,600	51.3	28.				
640	1.255 x .49	.6149	.3132	8	10.235	36,000	58,400	49.	27.8		.13	.049	.30
640	1.26 x 49	.6174	.2975	8	10.268	36,850	59,600	51.8	28.3				
641	1.284 x .49	.6291	.2932	8	10.01	39,500	62,700	46.6	25.		.14	.054	.42
641	1.270 x .5	.6350	.3319	8	10.075	40,500	63,700	47.7	25.9				
642	1.260 x .493	.6212	.2924	8	10.168	35,500	57,100	52.9	27.		.12	.047	.24
642	1.285 x .49	.6296	.2768	8	10.212	35,700	56,700	56.	27.6				
643	1.297 x .485	.6290	.3052	8	10.388	38,500	61,000	51.5	29.8		.15	.040	.30
643	1.270 x .485	.616	.2892	8	10.175	37,700	61,200	53.	27.				
644	1.254 x .498	.6245	.3095	8	9.875	37,700	60,300	50.4	23.4		.15	.055	.41
644	1.272 x .5	.6350	.3305	8	10.38	39,700	60,900	48.	29.7				
645	1.235 x .504	.6224	.3053	8	10.25	36,500	58,600	50.7	28.1		.13	.061	.33
645	1.232 x .505	.6221	.3273	8	10.21	35,900	57,700	47.3	27.6				
646	1.241 x .497	.6167	.3141	8	10.186	37,000	60,000	49.	27.3		.15	.050	.33
646	1.245 x .5	.6225	.3293	8	10.005	36,500	58,600	47.	25.				
647	1.240 x .454	.5629	2998	8	10.046	33,300	59,100	46.7	25.5		.12	.048	.38
647	1.245 x .454	.5652	.2668	8	10.163	33,200	58,600	52.8	27.				

TO TELL IRON FROM STEEL, IN SMALL PIECES.

Translated from Dingler's *Polytechnisches Journal.* By W. F. WORTHINGTON, U. S. N.

A new fracture ordinarily furnishes the means for classifying test pieces, but its appearance is not a sufficiently safe guide in dealing with good, fine-grained iron or very soft steel.

In order to effect the separation with ease and certainty in such cases, Walrand has given a simple method, page 531, "Proceedings of the Société des Ingénieurs Civils," Paris, 1883. It is by observing the fracture of the test piece after heating and allowing it to take a blue color.

The trial can be conducted in the following manner:

Take a test piece about twenty-five or thirty centimeters long and make a slight scratch about four or five centimeters from the end. Then heat one end slowly and uniformly to a dark red color (325° to 400°C.), and cool it in water. During the cooling, while the piece is still warm, it must be rubbed with a file from time to time, until the shining metallic surface laid bare, has assumed a dark yellow, or better, a blue color, when it is to be cooled quickly and completely.

The fractures of the piece broken at the mark, serve for comparison. Ordinary wrought-iron broken when cold appears fibrous or crystalline; but treated as above, its fracture is dull, irregular, and of short fibre. Hard and moderately hard steel are fine grained; after the heating and subsequent treatment, they have a shining, totally or partially, smooth fracture. Sweedish iron has only traces of fibres and is hardly to be told from soft steel; after treatment, the fibres become distinct, the smooth appearance is lost, and the iron becomes so much the more distinguishable from soft steel treated in the same manner.

MARSAUT'S SAFETY LAMP.—Museler's lamp has been hitherto thought to be perfectly safe in any explosive mixture of gases where there are no currents, but recent experiments seem to show that an explosion can be produced in from 1 to 5 times per hundred. This discovery explain the accident at Champagnac. Marsaut has accordingly added a second sieve within the horizontal diaphragm, and his new model has resisted more than 6,000 trials, under conditions in which all other forms have been found defective.—*Soc. des Ing. Siv.*, April, 1883. C.

REPORT ON THE TRIAL OF THE "CITY OF FALL RIVER."

By J. E. Sague, M. E., and J. B. Adger, M. E., with an introduction by Professor R. H. Thurston.

[Introduction by R. H. Thurston: During the winter of 1882–1883 the writer became interested in the designs of a steamer, then in course of construction by Messrs. W. & A. Fletcher, of New York City, in which were embodied devices which had never before, to his knowledge, been seen in combination. The vessel was a side-wheel steamer of the general form now familiar to all who are accustomed to travel between New York and the Eastern cities on the water routes traversing Long Island Sound. The hull is broad and shallow; the length is moderately large in proportion to breadth, and the lines are tolerably fine. The engines are always of the type introduced by the Messrs. Stevens a half century ago, and still peculiar to American steam navigation—the "beam engine." The propelling instrument is the paddle wheel, and this has, hitherto, always been of the radial type. The engines have always had single cylinders of long stroke, making a moderate number of revolutions, but, in consequence of their length of stroke, having a rather high speed of piston. Their consumption of good coal averages not far from three pounds per hour per horse-power, when furnished with boilers of economical construction. These boilers are, as a rule, of the "return tubular" type, the fire being made in a "fire-box" of rectangular sections, and the gases passing to the "back connection" through flues of moderate size, returning to the fire-box end through tubes of from three to five inches diameter, thence passing up into the "smoke pipe" through a "steam chimney" of sufficient height to dry the steam somewhat before it is sent to the engine.

The plan proposed by the Messrs. Fletcher was a combination of the Redfield boiler, the compound beam engine, and the feathering wheel. The boiler had been built by them before with satisfactory results; the compound beam engine had been constructed many years earlier, from the designs of the late Erastus Smith, a well-known and talented engineer, who has recently died.* The feathering wheel was introduced into this type of vessel as early as 1809—in the *Phœnix*—by Mr. Stevens, who also, many years later, used the same device on the steamer *Stockton*, but it had never been attempted to combine all these well known devices in one vessel. This the builders of the *City of Fall River* were proposing to do.

After consultation between the constructors and the writer, it was determined to make a careful trial of the boat, after its completion, to determine the gain actually realized by the introduction of the devices above mentioned, and the writer was authorized to see the work properly done. The

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* The first compound engines are, however, said to have been built by an engineer of a still earlier generation, Mr. J. P. Allaire.

results, if they should prove as favorable to the builders as was anticipated, were to be put in a form satisfactory to them and to the writer, and the latter was to see them put in proper form for publication.

The work of determining the efficiency was finally entrusted to Messrs. Sague and Adger, who had shown great interest in this matter, and who were known to be fully competent to do all that was likely to be required of them. The several trials were, therefore, made by them, under the orders of the writer, and the report was prepared by them, unassisted, and presented to the writer in the early summer of the present year, in the form of a graduating "thesis" at the Stevens Institute of Technology. It will be seen, on examination of the report, however, that it is not to be considered as the work of mere novices, in engineering of this character. Both of its authors had been fortunate in having a considerable previous experience in this kind of work, and both had proven themselves thoroughly expert. The report itself is the best evidence that the confidence placed in them was not misplaced.

The *City of Fall River* was not placed on the route until the spring trade had fairly commenced, and the consequence was that no time could be spared to take the boat off the route for any special preparation for these trials, and the engineers in charge of the work were compelled to do it under some difficulties. The trials were necessarily made at night, and involved, not only some very trying and difficult work, but a continual strain which, together with loss of sleep, combined to make the task one of no ordinary magnitude. The production of so complete a set of data, under the circumstances, is somewhat remarkable, and is not often paralleled.

The results, as given in the report, are extremely interesting, and seem to the writer to fully justify the judgment of the builders in determining to adopt this novel combination.

As will be seen, the fuel used is but about four-sevenths as much as is commonly demanded by steamers of this class, and the weight of good coal per horse-power and per hour is brought down to two pounds, where three and a half would have been expected. The boat makes her trips with great regularity in all weathers, and is vastly less impeded by a heavy sea than even much larger vessels having the radial wheel. The fastest time from dock to dock, ten hours twenty-two minutes, is very remarkable for so small a craft, and the speed attained in the face of a heavy blow is still more extraordinary.

The success of the boat is thoroughly complete, so far as can be judged by experience to date.

The work of measuring the quantity of feed used by the engines was greatly facilitated by the use of the Worthington meters, and the firm of H. R. Worthington & Co. is entitled to especial acknowledgment for the interest taken in the procurement and adjustment of the meters, and for their care in standardizing them before and after the trials. Appended to the report is a summary of all work done to date, including the later figures obtained by the W. & A. Fletcher Co.]

Hoboken, N. J., November, 1883.

INTRODUCTION.

The new freight steamer, *City of Fall River,* now plying between New York City and Fall River, Massachusetts, was built for the Old Colony Steamboat Company, and so designed as to be especially adapted to their trade. The boat required had to combine speed and carrying capacity, and at the same time to be of such form as to admit of rapid loading and unloading. After due consideration of economy and first cost, together with the special demands of the case, it was decided to build a side-wheel boat; and the urgent advice of Mr. S. Taylor, of the W. & A. Fletcher Co., of New York city, overcame all prejudice, and the novel and unprecedented combination of a compound beam engine and feathering paddle-wheels was fixed upon. Our attention was directed by Professor Thurston, of the Stevens Institute of Technology, Hoboken, to the exceptional opportunities afforded in this case for a thorough test of the coal and steam consumption; and this we were enabled to make, at his request, through the liberality of the designer and builders of the engine and the courtesy of the Fall River Line.

The construction of the engine is such as to admit of readily disconnecting the high-pressure cylinder, thus converting it into the simple type of beam-engine common in side-wheel steamers upon American waters, and, after making a thorough test of the compound system, we were enabled to find the steam consumption when working under the latter conditions, which is, we believe, the first time that such a determination has been made with this class of engine.

The object of the present paper is to analyze the performance of each part of the vessel so far as it is separable from the rest, and to compare the results obtained by actual trial with those determined by the best theory.

THE HULL.

The hull of the *City of Fall River* was built by Messrs. Montgomery & Howard, of Chelsea, Mass., from model and specifications prepared by Mr. George Peirce, Superintendent of the Old Colony Steamboat Company. It is of wood, copper-sheathed, and of the best material in every part. The principal dimensions are as follows:

Length on the load water-line	260 feet	
Length over all	273 "	
Breadth of beam on load water-line	42 "	
Breadth of beam over guards	73 "	
Depth of hold molded	18 "	
Draught of water, light	9 "	3 in.
Draught of water, loaded 600 tons	12 "	
Depth between deck, from top of plank shear to top of upper frame	11 "	

The keelson, bilge streaks and beams are of the best Georgia pine. The planking of white oak and yellow pine, and all fastenings are copper and locust-tree nails below the water-line, and galvanized iron and locust-tree nails above. A marked peculiarity is the absence of the usual "hog-frame," its place being supplied by a Howe truss. The upper chord of this truss is two hundred feet long, and supports the deck beams, while the lower chord is bolted to each frame of the vessel. The longitudinal rigidity given by this truss is further increased by a network of diagonal iron straps, fastened behind the clamp-streak to an iron belt plate, and running downwards to the floor timbers. The combination renders the vessel extremely staunch, and even when she is rolling considerably there is not the slightest creaking or straining. The freight space, between decks, is entirely enclosed, and the upper works not extensive; so that the resistance caused by a head wind is not a very serious matter.

DISPLACEMENT.

The principles applied in computing the displacements are to be found in Professor Rankine's "Shipbuilding," Chap. III., Sec. 1, *et seq.* We first computed from the ordinates, which are always half-breadths, the water sections at the various water-lines from the first up to the twelfth or the load water-line. Then taking these water sections as the ordinates of a new curve, upon a base equal to the distance between the load water-line and the lowest water-line, we arrived at the displacements up to each water-line in the series.

The following is the computation of the area of the twelfth water-line section, and will serve to show the process by which all the others were obtained.

The number of cross-sections being 33, the number of intervals was even, and we therefore employed the multipliers given by Simpson's first rule (Mill Work and Machinery, Part II., Art. 289):

No. cross section.	Ordinate.			Multiplier.	Product.		
	Ft.	in.	8ths.		Ft.	In.	8ths.
1	3	6	5	4	14	2	4
2	5	9	7	2	11	7	6
3	8	1	6	4	32	7	0
4	10	4	5	2	20	9	2
5	12	6	0	4	50	0	0
6	14	5	0	2	28	10	0
7	16	1	0	4	64	4	0
8	17	5	1	2	34	10	2
9	18	6	0	4	70	0	0
10	19	4	1	2	38	8	2
11	19	11	5	4	79	10	4
12	20	5	0	2	40	10	0
13	20	8	4	4	82	10	0
14	20	10	5	2	41	9	2
15	20	11	6	4	83	11	0
16	21	0	0	2	42	0	0
17	21	0	0	4	84	0	0
18	21	0	0	2	42	0	0
19	20	11	6	4	83	11	0
20	20	10	5	2	41	9	2
21	20	9	1	4	83	0	4
22	20	6	4	2	41	1	0
23	20	3	2	4	81	1	0
24	19	11	1	2	39	10	2
25	19	5	7	4	77	11	4
26	18	11	4	2	37	11	0
27	18	4	2	4	73	5	0
28	17	7	3	2	35	2	6
29	16	7	1	4	66	4	4
30	15	1	4	2	30	3	0
31	12	10	4	4	51	6	0
32	9	6	2	2	19	0	4
33	4	10	5	4	19	6	4
					3)1649	1	4
					549	8	8

549·708

7·5 = distance between the cross-sections.

4122·81 sq. ft. in half section.

2

82·6245 sq. ft. it 12th water line.

The other water line sections having been obtained by the same process, the sections were taken as ordinates of a new curve; and the number of intervals being even, Simpson's first rule was again applied to obtain the load displacement, as follows:

No. of section.	Area of section.	Multiplier.	Product.
Base.	3426·345	1	3426·345
1	4960·305	4	19841·220
2	5695·300	2	11390·700
3	6023·940	4	24095·760
4	6589·950	2	13179·900
5	6900·195	4	27600·780
6	7159·950	2	14319·900
7	7385·625	4	29542·500
8	7579·050	2	15158·100
9	7760·625	4	31042·500
10	7929·145	2	15858·290
11	8088·225	4	32352·900
12	8245·620	1	8245·620
			3)246054·515
			82018·1716

The interval between the water-lines is 1 foot; therefore we have the displacement equal 82018·1716 cubic feet, and taking 35 cubic feet equal to 1 ton, we have 2343.376 gross tons as the displacement up to the 12th, or load water-line. In this no allowance has been made for the "appendages," but from the data in the hands of the authors, it was estimated that the increase of the displacement at the load water-line due to these appendages would amount to only 7 tons, making a total load displacement of 2,350 gross tons.

In calculating the displacements up to the various other water-lines, the rules given in Rankine's Shipbuilding, (Chap. I, Art. 18 A), were used to obtain the volume between each water-line and the one immediately below it, and this volume was subtracted from the displacement up to the upper line of the two in question. For example, to get the displacement up to the eleventh water-line; to five times the area of the twelfth water-section, add eight times the area of the eleventh water-section and subtract the area of the tenth water-section; multiply the remainder by one-twelfth of the depth of the intermediate layer; the

product will be the volume of the layer; and this volume we subtract from the displacement of the twelfth water-line.

Area of twelfth section	8245·62 × 5 =	41228·10
Area of eleventh section	8088·225 × 8 =	64705·80
		105933·90
Area of tenth section		7929·145
Difference		12)98004·755
Cubic feet.		8167·063

This is now subtracted from the volume displaced up to the twelfth water-line, 82018·172 cubic feet, and the remainder, 73851·109 is the displacement up to the eleventh water-line; or, dividing by 35 cubic feet = one ton, we obtain 2110· gross tons as the eleventh water-line displacement.

In the same way the following displacements were computed for various drafts over the whole possible range for our boat.

Up to the thirteenth water-line	2581·130	gross tons.
Up to the twelfth water-line	2343·376	"
Up to the eleventh water-line	2110·031	"
Up to the tenth water-line	1881·308	"
Up to the ninth water-line	1665·037	"
Up to the eighth water line	1437·518	"

In these figures no allowance is made for the displacement of the appendages.

RESISTANCES.

In Rankine's Shipbuilding (Chap. V, Art. 150 *et seq.*), it is proven that, since a great part of the resistance of the water to the motion of a ship though it is exerted in an indirect manner, the computation of that resistance by a determination of the pressures exerted directly upon the several parts of her immersed surface becomes too complex a problem, and the approximate solution of which he there gives and which is here applied to our case is founded upon the two principles: the *equality of impulse and momentum* and *the equality of energy and work.*

The main causes of resistance are:

The Distortion of the Particles of Water;
The Production of Currents;
The Production of Waves; and
The Production of Frictional Eddies.

The resistance caused by the distortion of the particles of water is, Rankine says, inappreciable in the case of an actual ship, on account of the comparative slowness with which the distortion takes place, while it does become an important factor in the case of models.

The resistance due to the production of currents never acts upon a well designed ship; the form of which enables the particles of water to glide over the whole length, and then be left behind without retaining any appreciable motion.

The production of waves, up to a certain limit of speed does not cause a sensible amount of resistance, though beyond this limit the resistance increases very rapidly with the speed. Thus, according to Rankine, we have only to consider the resistance due to the production of frictional eddies.

This is due to the combined direct and indirect adhesion of the particles of water to the skin of the ship, which, together with the stiffness of the water, produces an infinite number of small whirls or eddies in the layer immediately adjoining the ship's surface. The velocity of the whirling particles bears some fixed proportion to that with which the particles glide over the surface of the ship. The actual energy, therefore, of the whirling motion imparted to a given mass of water at the expense of the propelling power of the ship, is proportional to the square of the velocity of gliding, *i. e.*, to the height due the velocity of gliding.

The velocity of gliding of the particles of water over a given portion of the ship's surface bears a ratio to the speed of the ship, depending on its figure, and the position of the portion of the surface in question; and the height due to the velocity of the gliding is equal to the height due to the speed of the ship multiplied by the square of this ratio. Further, the mass of water upon which whirling motion is impressed by a given portion of the ship's skin, while she advances through a unit's distance, is proportional to the area of that part of the skin multiplied by the above-mentioned ratio. Hence the resistance to the motion of the ship, due to fractional eddies by a given portion of her skin is the product of the following factors:

I. The area of the portion of the ship's skin in question.

II. The cube of the ratio which the velocity of gliding of the particles of water over that area bears to the speed of the ship.

III. The height due to the ship's speed, *i.e.* [speed in knots]^2 $\div$ 22·6.

IV. The heaviness of water [64 lbs. per cubic foot], and,

V. A factor called the *coefficient of friction*, depending on the material with which the ship's skin is coated and its condition as to roughness or smoothness.

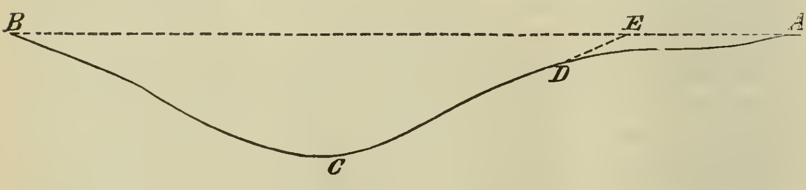
The sum of the products of Factors I, II, and III for the whole surface of the ship is called by Rankine, from whom this is taken, the *augmented surface.*

The resistance due to the formation of eddies may, therefore, be expressed in algebraic symbols, as follows:

$$\text{Eddy resistance} = f\,w\,\frac{C^2}{2g}\int q^2 ds.$$

in which $\int q^2 ds$ is the augmented surface, C is the speed of the ship, g is gravity, w is the heaviness of water, and f is a coefficient of friction, which, according to Weisbach (Rankine's "Shipbuilding," Chap. V., Art. 157), is for surfaces of clean painted iron, $f = {\cdot}0036$. Rankine uses this figure for iron ships, and says that the proper one for copper-sheathing is less, but that its exact value is not known.

The computation of the exact augmented surface would be a very complex and almost impracticable problem, and again an approximation must be resorted to. In Rankine's "Shipbuilding" (Chap. V, Art. 162) we find that the method pursued is to assume a figure nearly approximating to the real form of the ship. The figure chosen by him is the trochoid, and the following the formula employed:



The augmented surface is equal to the product of—

I. The length of the ship upon the plane of flotation, which corresponds to the length $A\ B$ of the trochoidal riband.

II. The *mean immersed girth*, found from the body plan as will be shown farther on, corresponding to the total breadth of the riband.

III. A *coefficient of augmentation*, deduced as follows:

The coefficient of augmentation for a trochoidal riband is

$$1 + 4\,[\text{sin. of greatest obliquity}]^2 + [\text{sin. of greatest obliquity}]^4,$$

the greatest obliquity meaning the greatest angle $B\ E\ D$ made by a tangent $D\ E$ to the riband at its point of contrary flexure D with its straight chord $A\ B$. For a ship the coefficient of augmentation is the mean of the above coefficients as deduced from the greatest angles of obliquity of the series of water-lines of the forebody, shown on the half breadth plan.

For the *City of Fall River* the following measurements were made, and the coefficients of augmentation obtained:

Number of Water-line.	Actual Measurement.	Sin.	Sin.^{2}	Sin.^{4}
2	5·03	·2515	·0632	·003994
3	4·96	·2480	·0615	·003782
4	5·00	·2500	·0625	·003906
5	5·12	·2560	·0655	·004290
6	5·12	·2560	·0655	·004290
7	5·27	·2635	·0694	·004816
8	5·39	·2935	·0726	·005271
9	5·57	·2785	·0775	·006006
10	5·75	·2875	·0826	·006823
11	5·95	·2975	·0885	·007832
12	6·14	·3070	·0942	·008874

The coefficient of augmentation for

Twelfth water-line.... .... 1·272011
Eleventh water-line.. 1·262603
Tenth.. .... 1·253509
Ninth .... 1·245138

To get the mean immersed girth, a drawing of the body plan, on the scale of $\frac{1}{2}$ inch to 1 foot, was made from the ordinates, and the half girths measured accurately for the 33 cross-sections. The mean value of these half girths was 10·57 inches, which multiplied by 2, on account of the scale, gave 21·14 feet for the value of the mean half girth, and this gave 42·28 feet as the mean immersed girth. This value is the mean immersed girth up to 3 inches above the tenth water-line. The coefficient of augmentation for that draft is 1·2558, and the length of the ship upon that plane is 260 feet; therefore, the *augmented surface* $= 260 \times 42{\cdot}28 \times 1{\cdot}2558 = 13804{\cdot}7$ square feet.

We are now prepared to compare the theoretical probable speed with that actually obtained, or to compute the horse-power theoretically necessary to give a desired speed.

On the double trip, made on the nights of the 9th and 10th of May, the average speed of the *City of Fall River* was 14.54 knots per hour, and the indicated horse-power, as computed from cards taken every half hour during the two runs, was 1616·58. The vessel was immersed on both nights to very nearly the same draft, 10 feet 7 inches, or 3 inches above the tenth water-line, for which draft the coefficient of augmentation has been calculated; 4 inches being allowed for depth of keel.

According to the rule (Rankine's "Shipbuilding," Chap. V, Art. 165), we multiply the indicated horse-power by the coefficient of propulsion, divide the product by the augmented surface, and extract the cube root of this quotient for the *probable speed in knots.*

The coefficient of propulsion established by Rankine for clean iron ships was 20,000, a figure which he arrived at by assuming the proportion of power wasted in the wasteful resistance of the propeller, in slip and through the friction of the engine, to be ·63 of the effective or net power employed in driving the vessel. The coefficient was found by him to be practically exact in a number of cases for iron vessels. He says, however, that, for copper-sheathed vessels, the coefficient of propulsion is unquestionably greater; but how much greater could not then be told owing to the scarcity of experimental data.

Messrs. Elliott & Lieb, of the Class of "80," Stevens Institute of Technology, in some researches in this direction, determined the coefficient of propulsion for copper bottomed vessels to be 23,500, and this coefficient is made use of in the following:

We then have, for the nights of the 9th and 10th of May, the average horse-power 1616·58, multiplied by 23500 equal to 37989630; and dividing this figure by the augmented surface, 13804·7 square feet, we get 2752. We then, according to the rule, extract the cube root of this number, and have as the probable speed 14·02 knots. Subtracting this from the actual speed, 14·54 knots, we have ·52 knots in favor of the vessel.

Again, on the double trip of the nights of the 3d and 4th of May, the actual average of speed obtained was 14·21 knots per hour, while the probable speed, calculated by the above given rule, was 13·96 knots; a difference of ·25 knots, again in favor of the vessel.

In these computations it is assumed that the tide and wind for the two trips balanced; but the error either way might be appreciable. Upon examining the lines of the *City of Fall River* and laying down on the half breadth plan [see dotted lines on the accompanying tracing] the rolling wave line constructed according to the principles stated in Rankine's "Shipbuilding," Part II, Chap. I, Art. 5, it was found that the lines were less sharp than the true trochoid, so that it might be expected that the Rankine theory would not apply exactly, but that the probable speed according to the theory would be greater than that actually obtained. The difference on the opposite side of the scale, as shown above, must therefore be due, either to some error in the coefficient of propulsion, or to the effects of wind and tide. That the latter might have accomplished it is shown by the following facts.

On the night of May 10th, the speed between Point Judith and the Gull Light was noted, and the horse-power of the engine at that time ascertained. From these obvervations the following comparison was obtained:

Actual speed	14·53	knots.
Probable speed	14·03	"
Difference	·50	"

At this time the tide was favorable and the wind hard against the ship. On the same night observations were made at Faulkner's Island and Statford Shoals and the figures obtained were:

Probable speed	14·03	knots.
Actual speed	13·45	"
Difference	·58	"

This difference is on the other side; but at this time the tide and wind were both hard against the ship. It is however the opinion of the authors, formed from information gotten from the pilots, familiar with the route, and from frequent observations during the trips, that in the case of the two double trips quoted above, the tide and wind on the outward and return runs balanced, or if anything were against, rather than for the vessel.

An error in the coefficient of propulsion might be due to the fact that the Morgan Feathering Paddle Wheel is more efficient than the forms of propeller used in the vessels upon whose performance the

above used coefficient was established; and also to some difference in the coefficient of friction for a new copper bottomed boat. These points cannot now be determined without going into a much more extended discussion than the scope of this paper will allow.

PADDLES.

The paddles used are those known as the Morgan Feathering Wheel, and are fitted so as to exert about the same action upon the water as a common radial wheel of twice their diameter. They are of great strength, and are designed especially to withstand the action of ice, which has hitherto been one of the principal impediments to the introduction of feathering wheels in these waters. Each bucket turns upon an axle forming part of the wheel frame; the motion being communicated to it, through arms upon its back, by rods connected to straps upon each side, which revolve about a large journal placed eccentrically to the shaft, and fastened to a truss frame built for the purpose on the guard. The principal dimensions are:

Diameter outside of buckets	25 feet 6 inches.	
Number of buckets,	12	
Width of each bucket		40 inches.
Length of each bucket	10 feet.	
Distance from centre of wheel-shaft to centre of eccentric actuating paddle levers		12 inches.
Length of arm on bucket from axle to lever pin		21 inches.

The feathering thus secured is only approximate; but by laying down the cycloid described by the wheel in going forward through the water, it will be seen, as stated by Rankine,* that the oblique action is too little to be considered in analyzing the efficiency.

Rankine, in treating of the action of propellers in general proceeds as follows.

Every propelling instrument, whether a paddle, screw, or pump, drives a ship by means of the forward reaction of the current which it sends backward. That reaction is transmitted through the propeller to the ship, and, when the vessel moves uniformly, is equal and opposite to her resistance.

(To be continued.)

* Machinery and mill work.

CORRESPONDENCE.

"THE CHEAPEST POINT OF CUT-OFF."

Editor of the Journal of the Franklin Institute:

SIR:—In the course of a discussion in the JOURNAL, under the title, "The Cheapest Point of Cut-off," I observe that I am accused of heresies, to which I must plead "Not Guilty!"

My views have been entirely misapprehended, as will be seen by reference to papers published long ago in the JOURNAL.

It is said (J. F. I, June, 1884, p. 407) by your contributor, that if he is right, the method of Rankine and the papers of others, including myself, "must be valueless." It is further asserted that other writers, including myself, "have deceived themselves, and perpetrated the absurdity of saying that you can save money by using more steam than is really necessary to do the work demanded." The former accusation is quoted from *The American Engineer*, of 1883, and both have been printed more than once.

I paid no attention to these statements when first published, feeling sure that the writer of these sentences would, in good time, discover and correct the errors. Their repetition makes it necessary for me to correct them for myself. Referring to my papers, already printed in the JOURNAL, it will be seen that, in that of May, 1882, I showed that the "cheapest point of cut-off" was determined by the solution of the "Designer's Problem" enunciated and defined by me thus:

"Given, the quantity of power required, to determine what ratio of expansion, and what size of engine, will give that power at minimum cost." . . . "To solve this problem, the engineer must know the cost of engines, boilers and appurtenances, and all items of running expense. Then, making the sum of both items of variable annual expense—those variable with size of engine and those variable with quantity of steam demanded—a minimum, the sum of those items and of all invariable expenses, *i. e.*, of the total running expense, becomes a minimum, and the problem is solved."

Turning to the enunciation of the same problem, in a later number of the JOURNAL, as given by my critic, I find the following: "The question equally at issue between myself and my critics is this: Do the constant charges have the effect of making the cheapest point of cut-off later than it would appear to be from a purely physical consideration? I have asserted, and believe I have proved, that they do not."

It would appear to me that I had myself anticipated this proof; and I may be permitted to say that my object in writing the paper of May, 1882, to which I have referred for a definition of the problem of the "Cheapest Point of Cut-off," was partly to present such a proof.

With regard to the second statement to which I am compelled to take exception, I need only quote from the same paper to show how far my views differ from those attributed to me, and to prove that I have not "perpetrated the absurdity of saying that you can save money by using more steam than is really necessary to do the work demanded."

There I defined what I have called the "Owner's Problem," which my critic correctly says is that of determining "how to make the best of an existing plant which is not adapted to the requirements of its work," as follows:

"*Given*, the size, power, and all items of cost, and running expenses, of a known plant of steam machinery, to determine what method of working the steam, *i. e.*, what ratio of expansion, will give the most work for a dollar."

Of this case, I remarked that, although the solution determines the ratios of expansion which "give more work for a dollar than the higher ratios" determined by the solution of the first problem, "they do not give maximum efficiency of capital," and go on to state the reason of this fact (Article 14, same paper). The same considerations had been already presented in the same paper, by me (Article 10) thus: "This problem is less frequently presented to the engineer than those already given" (those relating to the first case) "and is not the problem of maximum commercial efficiency; since this ratio, and the corresponding power of the engine, being determined, it will be found, on solving for maximum commercial efficiency, that another proportion of engine, with higher ratio of expansion, will supply the power now demanded at still lower cost. To this latter engine the last problem again applies. The practical conclusion to be drawn from the solution of the interminable succession of problems of this last character, which thus follow the first, is, that the largest amount of power possible should be entrusted to a single engineer, or crew of attendants, and placed under one roof, etc."

I trust that it is not necessary further to discuss this subject, to relieve myself from this misconstruction, and the singularly erroneous charge, to which I have been compelled to reply.

Professor Rankine's enunciation of the first problem is substantially the same as my own. He does not consider the second. His method, in my opinion, is not only not "valueless," but, properly modified to allow for cylinder condensation, becomes supremely valuable.

R. H. Thurston.

Hoboken, *N. J.*, June, 1884.

Book Notices

Topographical Surveying, including Topographical Surveying, by Geo. J. Specht, C. E.; New Methods in Topographical Surveying, by Prof. A. S. Hardy; Geometry of Position Applied to Surveying, by John B. McMaster, C. E.; Co-ordinate Surveying, by Henry F. Walling, C. E. Reprinted from Van Nostrand's Magazine. New York: D. Van Nostrand, 1884.

There is much of interest and of use in these four papers, which form No. 72 of Van Nostrand's Science Series. From Mr. Specht's paper, which should have been entitled "Stadia Measurements, and Leveling by Vertical Angles," the student may obtain a fair general idea of the principles and

practice of these methods, and of some of the instruments used in their employment.

The author goes somewhat out of his way to express his disapproval of Mr. Wellington's apparent contempt for contour-line maps. Mr. Specht appears to overlook the fact that Mr. Wellington's strictures are directed to "a contour map *of an ill-judged line.*" No doubt Mr. Wellington would agree with Mr. Specht as to the advantages of a carefully prepared contour map sufficiently extended to embrace *all* the possible lines.

Prof. Hardy's paper, entitled "New Methods in Topographical Surveying," is really an account of photographic topography as practised by the French engineers, notably, Messrs. Laussedat and Ducrot. The use made of the photographic process in Savoy and the Vosges, is referred to as illustrating the saving of time to be effected by its employment.

Mr. McMaster, in his introduction, inspires the reader with his own enthusiasm for the "New Geometry," or "Geometry of Position," which "differs essentially from the Old Geometry, or Geometry of Measure in three particulars: in the simplicity and paucity of its elements, in the total absence of the idea of measure and all metrical relations, and in the great generality and comprehensiveness of its principles and problems." After giving an explanation of the broad principles of the new science, the author illustrates how it may be employed in the solution of surveying problems, such as passing an obstacle, or measuring the distance of an inaccessible point. We fear that, in view of the number of points to be located and borne in mind in employing this method, and the consequent liability to error, most surveyors will prefer the old and apparently simpler methods of laying off three angles of 60°, or four of 90°, in the one case, and of measuring a base line and the two angles at its ends in the other.

As we understand Mr. Walling's paper, it is a suggestion that our states should, with the aid furnished by the government trigonometrical surveys, be divided into rectangles of convenient size, and with sides running north and south and east and west, in order that points may be located by means of their distances north, east, etc. of these lines. The details of the proposed method will, we believe, be found to have been carefully concealed by the author in his descriptions.

Indeed, all of these papers strikingly illustrate the facility with which an author, entirely at home with his subject, may fail to realize that his readers are presumably less familiar with it, and may indulge in technicalities which convey to the uninitiated no satisfactory idea.

The obscurity arising from this inability of the author to put himself in his reader's place, is increased by the topographical errors, which abound throughout the volume, except in the last paper. Some of these are simply amusing, as where we read of the "*total* (instead of *focal*) length of the object glass." "The *largest* (instead of *target*) I use." "Both . . angles are thus determined from the *views*" (views), and "The *positions* (positives) thus obtained, though less *distant*" (distinct). Other errors, occurring in the formulæ, are annoying if not insurmountable. For instance, in Mr. Specht's paper, a half-page of equations is made to depend upon the similarity of two dissimilar triangles. J. C. T.

RECENT LOCOMOTIVES - Illustrations, with Description of Specifications and Details of Recent American and European Locomotives. Reprinted from the Railroad Gazette, 1883. New York: Railroad Gazette.

The work consists of reprints from the *Railroad Gazette*, some of which have been taken from foreign sources.

The first forty-six pages contain descriptions of the engines which are shown in the succeeding plates.

Specifications of the Standard American Locomotive, as built by the Baldwin Works (1877), and by the Grant Works (1873), and of the Mogul Freight Locomotive, as built by the Baldwin Works (1872), are given together with the principal dimensions, and in many cases the weights of the engines shown.

The engines are the principal ones built in the last twelve years, including such as the following, of American build, viz.: Baldwin Standard, Grant Standard, N. Y. C. and H. R. Railroad, Philadelphia and Reading, Pennsylvania, Mogul, Consolidation, Ten- and Twelve-wheel and Tank Locomotives.

Among foreign engines there are a number of English ones, and representatives of France, Sweden, Belgium and Switzerland.

A description and plates are also given of the Fontaine, Fireless, Forney and an English Compound Locomotive, and engines for the Elevated Railroad and for Street Railroads.

Descriptions and cuts are also given of the Wootten and Belpaire fireboxes, Hill's fire-box door, the water-table used on the N. Y. and H. R. Railroad, Allen's valve, and the double valve used on the Central Pacific road.

The Joy valve gear is shown in detail, as is also the Walschaert gear with the Allen link.

A better selection of engines to represent types could not have been made, had it been the original intention to issue the matter in book form.

The descriptions are for the most part short and to the point, giving the reader the information most generally desired.

The plates are clear and distinct, having in many cases dimensions and sections of the various parts.

The work gives, in a convenient form, much information about locomotive engines and boilers, and should be in the possession of all those interested in the subject.

There is only one criticism to make, and it applies to the majority of works of this class. Having the description in one part of a book and the plates in another makes the use of the book inconvenient. H. W. S.

THE WATCH AND CLOCK MAKER'S HANDBOOK, DICTIONARY AND GUIDE. By F. J. Britten. London: W. Kent & Co.; New York: E. & F. N. Spon, 1884.

Horology has a peculiar charm, and good literature on the subject is welcomed and appreciated by all who are devoted to that branch of applied science. We recognize in this volume a work belonging to this class. However, to review this book would be but to repeat what was said in this JOURNAL, Aug., 1882, about the fourth edition of the "*Watch and Clock*

Maker's Handbook," which was by the same author, and was in substance, with some new matter, identical in contents, but not in arrangement, with the present book.

What makes this volume especially desirable is the manner in which it presents its material. All subjects are given in alphabetical order, which facilitates ready reference to any desired subject, and without loss of time.

The technical terms from the French and German languages are given along with the English, and will be serviceable in many instances where the knowledge of them is needed.

A commendable and pleasing feature of the book is, that it contains short biographical sketches of men who have aided the advancement of horology in any of its branches.

One defect of the book, which it has in common with other works on clock making, is its failure to give to electric clocks a more deserved consideration. This omission is inexcusable in view of the prominence which various systems of time telegraphy have won before the public, and the technical interest involved in their perfection. The bare mention of their existence will not do any longer in a work of this kind. The indications are, at the present time, that the usefulness of the electric clock will have to be demonstrated more through the efforts of the electrician than by those of the experienced clock makers, although the assistance of the latter is much needed. Notwithstanding the prejudice against the new mode of giving the time, and the embarrassments attending the introduction of the electric clock occasioned thereby, it is destined to win a deserved success in the near future.

This defect, however, is one common in all similar works, and it does not detract anything from the excellency of the matter which is contained in this book. L. H. S.

Bulletin de la Societe Internationale des Electriciens. Tome 1. 1884.

The Committee propose to issue ten or twelve numbers of the *Bulletin* each year, and ask the co-operation of all members of the Society in transmitting documents, which they think will interest the learned or the industrial world, or will help to popularize the knowledge of theoretical electricity and its applications. The subscription price, within the limits of the Postal Union, is 27 francs per annum. The annual fee, for ordinary membership, is 20 francs. The aims of the Society are: 1. To collect information and documents concerning the progress of electricity. 2. To promote the popularization and development of electricity, by writings, publications, and financial aid to successful investigators. 3. To establish and maintain intimate friendly relations among the members. C.

The Principles and Practice of Electric Lighting.—By Alvan A. Campbell Swinton. New York: D. Van Nostrand, 1884.

This small octavo volume is not intended to be a treatise for specialists, but rather pretends to fresh and reliable information for the users of electric

lighting and for the general scientific public. It treats of the elementary theory of electric lighting, of mechanical and electrical measurements necessary, of the various sources of power and then of the dynamo electric generators themselves; of the forms of arcs and resistant lamps, secondary batteries, peculiarities of the various electric lighting systems, and finally of the applications of this form of lighting, with its advantages and cost.

The matter is presented in an unpretentious manner, and, what is most gratifying, the descriptions of obsolete and impracticable appliances are omitted. If called upon to recommend a volume for first reading on machinery relating to electric lighting we could scarcely find one so likely to meet the wants of the majority of inquirers. M. B. S.

Franklin Institute.

[*Proceedings of the Stated Meeting, held June* 18, 1884.]

Mr. William P. Tatham in the chair. Present, 179 members and 14 visitors.

The minutes of the May meeting of the INSTITUTE, of the Board of Managers, and of the several standing committees were reported and approved. Thirty-nine (39) persons were elected members at the last meeting of the Board.

On the recommendation of the Board of Managers, the following gentlemen were unanimously elected as honorary members of the Institute:

Sir Alexander Grant, Bt., Principal of the University of Edinburgh, 21 Lansdown Crescent, Edinburgh; Professor Thomas C. Archer, Director of the Edinburgh Museum of Science and Art, Edinburgh; Professor William Swan, LL.D., F.R.S.E., Ardachaple, Helensburgh, President of the Royal Scottish Society of Arts; Edward Sang, Esq., LL.D., F.R.S.E., 6 Molendo Terrace, Edinburgh, Secretary of the Royal Scottish Society of Arts.

Mr. Charles J. Quetil read a paper descriptive of a "Triangular Suspension Truss," which he had devised, and which is proposed for elevated railroads, bridges, roofs, viaducts, etc. The paper was discussed by Mr. Hugo Bilgram and the author. It has been referred to the Committee on Publications.

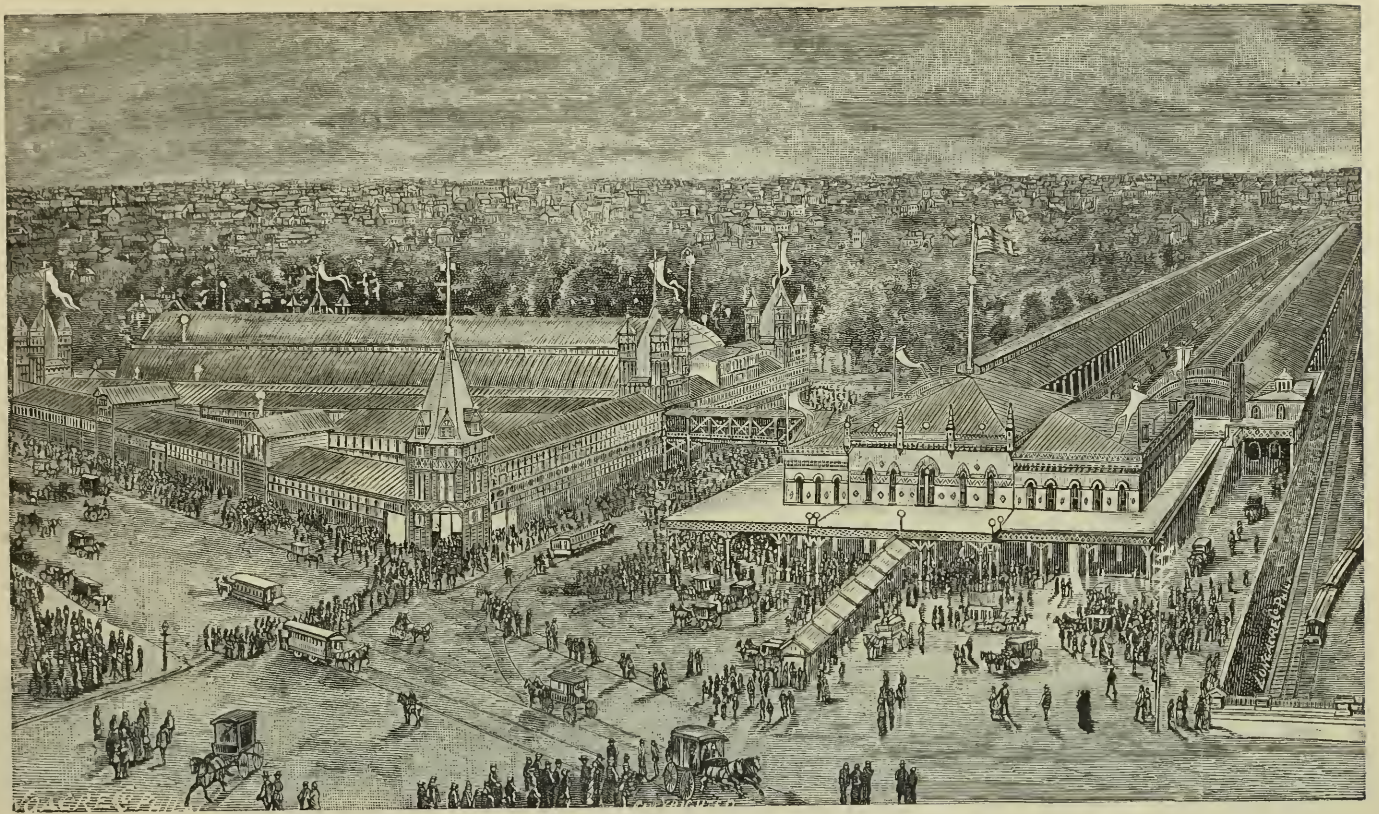
Mr. S. Lloyd Wiegand read a paper explanatory of the phenomena accompanying "Tests by Hydrostatic Pressure." The paper was discussed by Messrs. J. W. Nystrom, Bilgram, and the author. The paper has been referred for publication.

Mr. J. W. Nystrom, described and showed in operation, a magnetic engine of his invention.

The Secretary's report embraced some remarks on the future water supply of the city of Philadelphia, based principally upon the report of Col. William Ludlow, Chief of the Water Department.

Several mechanical inventions were shown and described. Adjourned.

WILLIAM H. WAHL, *Secretary.*



BUILDINGS OF THE INTERNATIONAL ELECTRICAL EXHIBITION OF THE FRANKLIN INSTITUTE.

JOURNAL

OF THE

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FOR THE PROMOTION OF THE MECHANIC ARTS.

Vol. CXVIII. AUGUST, 1884. No. 2.

WIRE TRIANGULAR TRUSS.

By Chas. J. Quetil, C. and M. E.

[Read at the Stated Meeting, Wednesday, June 18, 1884.]

Gentlemen: I have the honor to call your attention to a new truss of which I had the idea, and which for permanent loads dispenses entirely with horizontal strains on the chords and allows to support a moving weight with lighter material than is generally done. It is made with steel wires isolated or united together in a form of cable or rope. I would recommend its application to different kinds of structures, such as elevated railroads, bridges, roofs, viaducts, acqueducts and others. To give you a general idea of the truss, I would say, take one-inch board and draw on it a rectangle 10 inches long and 5 inches high. (Fig. 1.) Call the four corners *A, B, C, D*, and at each of them drive a small wood screw. At the top screws *A* and *B* fasten the two ends of a wire, No. 20, of such a length that pressing it with the finger it deflects a little less than two inches. Pull on the centre of it with a spring balance, and when the deflection is two inches note the number of pounds indicated. Suppose it to be eight pounds. Fasten to the bottom screws *C* and *D* the two ends of another wire of same length as the first and with a third wire brace them together so that the deflection of each be two inches. You have now a truss almost rigid for any pull or weight coming on it less than eight pounds. If there is any deflection it will be so much below the quantity allowed in good con-

struction, which is the $\frac{1}{1500}$ of the span, that practically it will not amount to anything. I can increase considerably the rigidity of the truss with a wire having its ends fastened in A and B and passing under wire $C\,E\,D$ supporting the apex E and resisting its deflection when the load comes on the centre of the span. Let you suppose now the fixed points to be bolts put through posts, the posts 10 feet apart from centre to centre and the deflection of the wire 2 feet. Suppose a whole line of posts put 10 feet apart from centre to centre and supporting one line or two parallel lines of light iron or steel rails or of T iron, the centre of those rails or T iron acting as such, being supported in the centre of the span of 10 feet by the rigid truss and you will have an idea of my elevated railroad. Half of the weight carried on the span by those rails will go on their centre and by means of short vertical posts will come on the truss. In my railroad the span of 10 feet

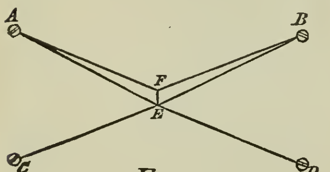


Fig. 1.

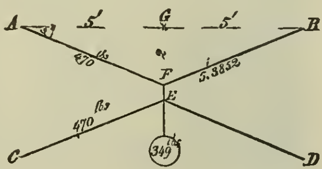


Fig. 2.

affords room for two small cars 4 feet long each and consequently the weight coming on the centre of the rail and which has to be supported by the truss is the weight of one car loaded. If the top wires, by means of an iron rod and nuts, have been braced in advance with the bottom wires, to a rigidity equal to the one they would get with the weight of one loaded car suspended to them, they will be kept to that rigidity or tension by the lower wires having the same tension with the same deflection, and when the weight of the loaded car will come on the centre of the rail, the effort causing that rail to deflect will be only half of that weight, because as soon as the loaded car presses on the top wires of the truss, the bottom wires are relieved and have a tendency to contract, their action on the top wires ceasing. Between those two actions, the weight pressing on them from the top and the lower wires ceasing to press on them, the top wires will deflect half the quantity they would if there was no truss and the loaded car was suspended to them. Consequently if in moderate weather, at a temperature I suppose of 45° Fahrenheit, the trusses have the required

rigidity for not deflecting more than $\frac{1}{1500}$ of the span or 0·08 inch when the weight of one loaded car comes on them, it is only at an elevated temperature of 120° Fahrenheit, for example, that the deflection may increase in a way worth to be observed and attended to. We are going to find what will be the increase of deflection and the way to keep it in proper limits. Let us examine in a general way the effects of variations of temperature upon the truss. Suppose we have a Bessemer steel wire No. 6 of the gauge of the Trenton Iron Co. It has a diameter of 0·190 inch and this commonly in the market will break under a pull of 2,875 pounds.

Suppose we have a truss *A, F, B, C, E, D,* made with such wire. (Fig. 2.)

The distance between the fixed points *A* and *B* is 10 feet, and the deflection of the wire 2 feet or $\frac{1}{5}$. Let us suppose that at a moderate temperature of 45° Fahrenheit, it has been found that the deflection 2

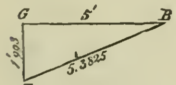


Fig. 3.

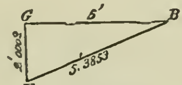


Fig. 4.

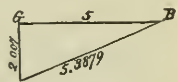


Fig. 5.

feet takes place when a weight of 349 pounds is suspended in *F* to the top wire. Then the tension on the wire is found by the formula

$$\frac{Wl}{4d} \text{Sec. } \varphi = \text{Tension, or } \frac{349 \times 10}{8} \times \frac{5{\cdot}3852}{5} = 470, \text{ to be } 470 \text{ lbs.}$$

Suppose now that a decrease of 75° takes place in the temperature, the thermometer falling to 30° below zero. Let us consider the top wire *A, F, B.* If it was free to contract, if it was not prevented by the lower wires, and if the weight 349 pounds was suspended to it, it would contract, raising that weight with it. As a steel wire, according to Faraday, expands or contracts 0·00000661 of its length for every change of 1° in the temperature, it would contract 0·0641 inch and its length instead of being 10·7704 feet, as it is at 45°, would be 10·7651 feet, and the deflection of the wire would have become 1·993 feet. (Fig. 3.)

With this new deflection the tension of the wire with the weight suspended to it would be 471·27 pounds. But when the truss is formed, the top wire is not free to contract and take a smaller deflection. The bracing rod *E F* prevents it. On the contrary, that rod itself has a tendency to contract by the cold and this has to be taken into account.

Let us see first what will be the contraction of the rod for 75 degrees fall in the temperature. Iron expands or contracts 0·00000698 of its length for 1° change in the temperature. Let the rod be 9 inches long and $\frac{1}{2}$ inch diameter. For 75° fall in temperature it will contract 0·00039 feet and become 0·7496 feet long. This would increase the deflection of the wire by 0·000195 feet at each end of the rod, so that at 30° below zero the deflection of the wire would be 2·0002 feet. The increase of tension of the wire in the truss at 30° below zero would be then precisely the increase of tension it would have if from the deflection 1·993 feet and the corresponding tension 471·27 found before, it it was brought by forcible pulling to the deflection 2·0002 feet. With deflection 1·993 feet the length of the wire is 10·765 and with deflection 2·0002 feet its length is 10·7706 feet. (Fig. 4.) It would consequently extend 0·0056 feet or 0·0672 inches. Then the increase of tension can be calculated and is $\frac{0{\cdot}0336}{5{\cdot}3825 \times 12} \times 35500000 \times 0{\cdot}02835 = 523{\cdot}54$ lbs. Then the wire of the truss at — 30° will have 471·27 + 523·54 = 994·81 pounds tension. As it was 470 pounds at 45° it has increased 524·81 pounds, and this has produced on the rod an extra tension of 390 pounds, causing it to extend 0·000666 inch, or 0·000055 feet. As the cold has a tendency to contract it 0·00039 feet, it will finally contract 0·00034 feet or 0·00017 feet at each end. The deflection will become 2·0026 feet, and the extra tension of the wire will be 523·54 pounds. Then the wire at — 30° will have 995 pounds tension, having yet a safety of $\frac{2875}{995} = 2{\cdot}88$, say 3, which is quite sufficient for wire. The limit of elasticity of that wire is between 1,438 pounds and 1,916 pounds, and the danger of its being broken is only when the tension approaches that limit of elasticity.

I will now show that at a moderate temperature of 45°, if there was no truss, and if the top wire had a deflection of 2 feet with the weight 349 pounds suspended to it, that wire would contract so as to have only 1·993 feet of the deflection if the weight was taken off. Because that weight gives the wire 470 pounds tension stretching it 0·06033 inch, or 0·0053 feet. Consequently that weight of 349 pounds suspended to the wire has brought it from the length 10·7650 feet to the length 10·7704 feet, increasing its deflection from 1·993 feet to 2 feet, or 0·007 feet, or 0·084 inch. And when the truss is formed in the conditions said above, if the weight 349 pounds be sus-

pended to the truss or presses on it, the deflection will be only 0·042 inch, or 0·00035 of the span, instead of 0·00066 allowed in good construction.

Let us see now what takes place when the temperature increases. For 75° increase of temperature the wire, which is 10·7704 feet long at 45°, will increase 0·000496 of the length, or 0·0053 feet. It will become 10·7757 feet long, the weight 349 pounds pressing on it. This corresponds to a deflection of 2·007 feet. (Fig. 5.) Under that deflection the tension of the wire, under the load 349 pounds suspended to it, will be $468\frac{1}{2}$ pounds. The deflection 0·007 feet is equal to 0·084 inch, or the deflection allowed in the beam, which is 0·08 inch, or the $\frac{1}{1500}$ of the span. Consequently the deflection of the rails worth noticing will only take place in very hot weather, and at 120° it will not exceed 0·08 inch, or the $\frac{1}{1500}$ of the span. The preceding calculations show that a safety $\frac{2875}{470} = 6$ is sufficient to allow the wires to resist their greatest tension in the extreme cold weather with a safety of 3 and to prevent them having in extreme hot weather a deflection greater than $\frac{1}{1500}$ of the span. With such a safety of 6, the elevated railroad, once established, need be regulated neither for cold or hot weather; but for bridges it is different, because you have then to use wire cables, more expensive than single wires. I intend building bridges on the system of my truss with safety of only 3, and to regulate them by hand, or have them regulate themselves, for all changes of temperature.

We have seen that for a span of 10 feet a wire No. 6, with a deflection of 2 feet, braced with another wire of same number, same length and same deflection, will form a truss which at all temperatures will support safely the weight of 349 pounds pressing on it. Suppose, now, that we have four such wires, No. 6, two on one side of the posts and two on the other side, passing over the same bolts through the posts, and braced with four lower wires, of same number, length and deflection, and we will have a truss formed with eight wires, which will support safely at all temperatures a weight equal to four times 349, or 1,396 pounds, and in supposing the rail used to be an iron one, weighing 16 pounds to the yard, or 54 pounds for the span of 10 feet, only the half of the weight of that rail, or 27 pounds, would have to be deducted from 1,396 pounds to get the moving weight, 1,369 pounds, that such a truss would support safely on its centre. An elevated railroad built with such a truss would support, then, safely, at all temperatures, a train formed of small cars, 4 feet long,

weighing each, loaded, 1,369 pounds; and, as each of those small cars, empty, would weigh about 250 pounds, they could be each loaded with 1,120 pounds, or $\frac{1}{2}$ ton of ore. A train of 40 of these small cars would be 200 feet long, and would carry 20 tons of ore, and the expense to build such an elevated railroad would be, on uniform ground, less than $3,500 a mile. Let two such trains, containing each 20 tons, be run in one hour from the mine to the mill or to the shipping point, and 400 tons will be transported in a day of 10 hours.

I will now explain why I make the spans only 10 feet instead of 20 feet or more. It is because, by so doing, I get tensions which allow me to use isolated wires costing five times less for same weight than a rope of equal strength. For example, we have seen that on a span of 10 feet, with wire No. 6, having a deflection of 2 feet, I can run safely two cars weighing each, loaded, 1,369 pounds, the truss supporting, besides, the weight of the rail. On such a structure the posts are 3 inches square, and their top is 5 feet above the ground. What would take place if the span was 20 feet instead of 10 feet? (Fig. 6.) The weight supported by the truss at its centre will be then the weight

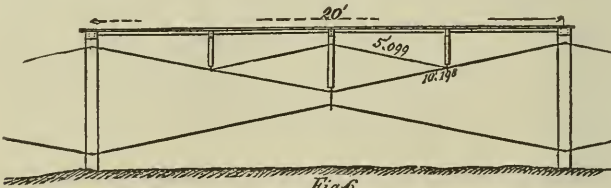


Fig. 6.

of two loaded cars, 2,738 pounds + 54 pounds, half the weight of the rail, total 2,792 pounds; and, if I do not want to increase the expense of the posts by increasing their length and size, in order to keep a deflection of $\frac{1}{5}$, I shall have to keep the same deflection, 2 feet, I had before, and have $\frac{1}{10}$ deflection. This increase of span, of weight and that decrease of deflection, is going to make the tension on the wire nearly four times greater than it was before; for it will be now 1,780 pounds on each of the four top wires, and the same on the lower ones, when it was before only 470 pounds on each. To have a safety of 6, each of the wires of the 20 feet span must not break under a pull less than 1,780 × 6, or 10,680 pounds. We come then to an extra size of wire, for wire No. 0, Birmingham gauge, breaks under a pull of

8,999 pounds, and we have to use steel rope. We would have to get 8 of them, of $\frac{1}{2}$ inch diameter. That would be a length of 163 feet of $\frac{1}{2}$-inch rope, costing $22 and weighing 57 pounds, while with two spans of 10 feet I have only 172 feet of No. 6 wire, Trenton Iron Co. gauge, weighing $16\frac{1}{2}$ pounds and costing 87 cents. It takes 10·44 feet of No. 6 wire to weigh one pound, and it costs $5\frac{3}{10}$ cents per pound; while $\frac{1}{2}$ inch rope costs 15 cents per foot, or $13\frac{1}{2}$ cents with discount of 10 per cent., and weighs 0·35 pound per foot. Furthermore, if I want to use the same 16-pound rail on the 20-foot span, I have to support it every five feet, and this will necessitate an additional length of rope equal to 21 feet and costing $2.84, bringing the total cost of rope to $24.84, while the No. 6 wire cost only 87 cents. If it is considered that the post economized by the 20-foot span cost only 20 cents, with the five holes drilled in it, it will appear at once how it is cheaper to have 10-foot spans instead of longer spans. The increase of length of span and the use of ropes makes the cost twenty times

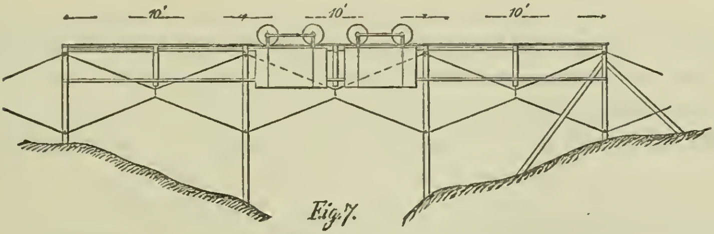


Fig. 7.

greater, and the advantages of my invention in using wires instead of wire ropes or cables, unless compelled to do it, and in reducing the spans to 10 feet, are self-evident.

I will now give a brief description of my elevated railroad or tramway applied to mines. (Fig. 7.) It is built with only one line of posts, generally 3 inches square and 7 feet long. They go $1\frac{1}{2}$ feet in the ground, and their top is consequently $5\frac{1}{2}$ feet above the ground. They are put 10 feet apart, from centre to centre, as I said before, and are braced at their bottom part by 2 braces, whose tops are joined to the posts by a bolt, and whose feet rest in slots cut in a plank, 2 feet long, 9 inches wide and 2 inches thick, which has a rectangular hole cut in it, through which passes the post. (Figs. 8 and 9.) This not only braces the post, but brings on the surface of ground covered by the plank the whole weight coming on the post. The posts are made besides perfectly

steady by wires fastened to them, and also to stakes driven hard in the ground, or to rock; and every fifty feet they are braced in the direction of the road. This line of posts will support, according to circumstances and wishes, either one single rail or two or three rails. I will give a description of the tramway with only one rail.

Fig. 10.

Fig. 8.

Fig. 9.

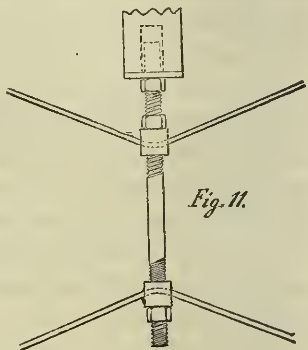


Fig. 11.

I calculate to use a 16-pound iron rail, most generally, in lengths of 20 feet, the ends of which will meet on a post and will be received and supported there by a casting fixed on the post.

The wires will be anchored at the start, and run over the bolts, top of the posts, the top wires will, while the bottom wires, equal in number, will pass below the bolts, foot of the posts. They will be braced together in the centre of the spans by an iron rod, with 3 nuts and two small

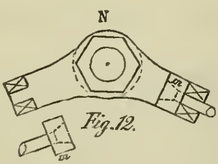


Fig. 12.

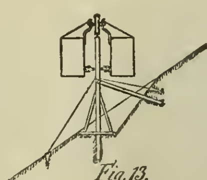


Fig. 13.

castings called wire pressers. Fig. 10 and Fig. 11.) Their deflection, calculated in advance, and which is 2 feet, and a loaded car run on the centre of each span, will indicate how they are to be braced. The wire can be run in lengths of 500 feet, 1,000 feet, or even one mile before being stopped. Where it is stopped the end of the wire is riveted in a small piece *m*, of steel casting, and held in another piece *N*, of steel casting, fixed on the bolts crossing the posts. (Fig. 12.) That piece holds the ends

of the wires stopped, and also those of the new wires. The rails are supported in centre of the span by a casting fixed on a short post, whose end has fixed on it a cast iron washer, resting on the top nut of the bracing rod. That bracing rod goes itself two inches in the post, which has a hole drilled in it to receive it. When the loaded car comes to the centre of the span, its weight presses on the top nut of the bracing rod and through it goes on the truss which receives it. That top nut allows also to give the rail a slight camber. The

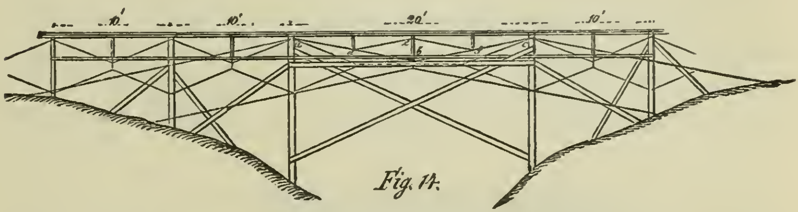


Fig. 14.

material for such a tramway, carrying trains of 10, 20, or 30 tons or more cost $2,000 a mile. The labor to work that material in the shop and put it in the field on uniform ground, cost $1,000 per mile; total cost of the tramway $3,000 per mile. This estimate is made with a supposition that the timber, sound and sawed to given dimensions, cost $30 per thousand feet, B. M., as it is generally in the West, and that

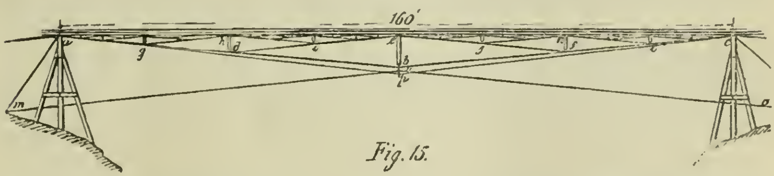


Fig. 15.

the iron, cast iron and labor cost as they generally do in Philadelphia. In places where the prices are such, I offer to furnish the material per mile, and put it up for $4,000 per mile. The small cars, holding a half ton each, will cost $25 each, and if the railroad runs by gravity, the cost of the rope, sheaves and machinery has to be added. That cost would be about $600 to $800.

The form of the cars is as shown in Fig. 13. They are formed with two boxes, hanging below the wheels, and 4 feet long, 16 inches wide, and 2 feet and 3 inches high. Each box has in its bottom part and

in its middle a small cast iron wheel, 5 inches diameter, running on a piece of *T* iron fixed on the posts. The diameter of the car wheels is 12 inches, and each car has only two wheels. They empty by the movable bottom. This elevated railroad is principally advantageous in countries where the ground is a little rolling or broken, because with it the grading can be saved. It is also very economical when the railroad has to run along a side hill, because I save the whole of the grading necessary with an ordinary railroad.

I will now explain how I would build a bridge with that same system of triangular truss. First, if the span is 20 feet I will give it this disposition. (Fig. 14.) Suppose I have a 50 feet bridge to build, I can, by lengthening the posts and making them 6 or 7 inches square, if necessary, have three spans of 10 feet and one of 20 feet. By supporting

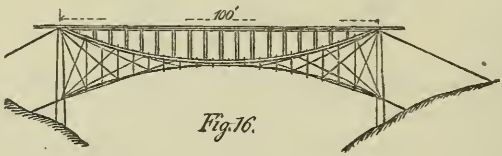


Fig. 16.

the points *d* and *f* of the wire *a, d, b, f, c,* by another wire *d, e, f,* and putting two small posts in *d* and *f,* the rail will be supported every 5 feet the same as in the 10 feet span.

If I have to build a bridge 160 feet long in one span, with a deflection of 8 feet, or $\frac{1}{20}$, I will build it as shown in the sketch. (Fig. 15.) The cables *a, g, d, b, f, l, c* and *m, n, o* being braced together by the bracing rod *b, n,* and made rigid, I will support the points *d* and *f* of the top cables by a wire *d, e, f,* brought itself to sufficient rigidity. Then support the points *g, i, j, l* by other wires, *g, h, i, j, k, l* brought also to sufficient rigidity, and so on, so as to support the rail every 5 feet, as in the span of 10 feet, or every $2\frac{1}{2}$ feet, if necessary. Bridges of 1,000 feet spans can be built very cheap, light and strong on this plan. In adding to the bridge the cable *a, p, c,* of same diameter as cable *a, b, c,* and pressed by a wire presser and a nut on the prolongation of bracing rod *b, n,* I will reduce still more the deflection in a very large proportion, and by means of a couple of springs on the rod *b, n,* the bridge will regulate itself at all temperatures.

Another bridge could be built with two cables, having the parabolic form and in an inverted position (Fig. 16), as was suggested by Mr.

Leblanc, a French chief engineer of road and bridges, but to make such a bridge perfectly rigid it would be necessary to brace it as shown on the sketch, and then it would be more expensive than the one I designed with the triangular truss of my invention.

The system proposed by Mr. Leblanc of two cables of a parabolic form in an inverted position, and braced together at regular intervals by vertical rods, would not, in my opinion offer sufficient rigidity. I improved it in bracing the truss, as shown on the sketch, but it makes

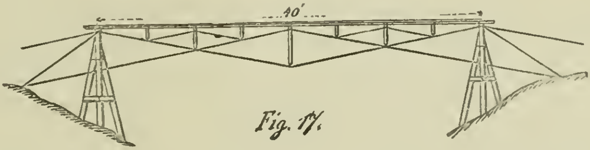


Fig. 17.

it more expensive, although for long spans it is, on account of its lightness, cheaper than the ordinary truss bridge. Such a suspension bridge would have all the rigidity necessary to allow a train of cars to pass over it, and would have but very little deflection. I prefer, however, the triangular truss, thinking it cheaper. (Fig. 18.)

I will conclude, saying that in the triangular truss one set of cables might be in the position *A, B, C,* and the other set in position *D, E, F,* the cables being braced together top and bottom of post *E, B.* This might be advantageous in some cases. This disposition applies also to the 10 feet span triangular truss. Fig. 17.)

Fig. 18 shows a suspension bridge (system Leblanc) improved by me, and designed for a mining company. It crosses the river with two spans, one of 275 feet, the other 137·5 feet. On the span of 275 feet

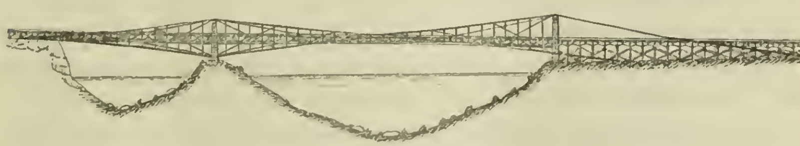


Fig. 18.

it is able to support, besides its own weight, 56 cars weighing each, loaded, 1400 lbs. Total 78,400 lbs. of moving load, which will not give the bridge over two inches deflection. Deflection $\frac{1}{20}$. The bridge supports on its own length $52\frac{1}{2}$ tons of moving load. There are four

top cables $2\frac{3}{4}$ inches diameter, and four lower cables $1\frac{7}{8}$ inch diameter. The superstructure of the 275 feet span weighs 64,095 lbs.

The total cost of the suspension bridge $412\frac{1}{2}$ feet long, including towers and masonry for anchoring is $12,000.

Philadelphia, June 7, 1884.

DISCUSSION.

MR. HUGO BILGRAM:—I can see that the addition of the lower chord reduces the amount of inflection due to a certain change of load, but I fail to see that this truss is any stronger than the same truss minus the lower chord, *i. e.*, I do not see how the addition of the lower wires contribute to the ultimate strength of the truss.

MR. QUETIL:—In reply to the question of Mr. Bilgram, if the truss would not be just as good without the lower wires, and bracing simply the top wires in the ordinary way, I answer in the negative. The question is equivalent to asking me if my truss presents any advantage over the ordinary triangular truss. This I can answer affirmatively. Evidently the gentleman does not see that the great advantage of my truss, besides the one it has of reducing the deflections more than the ordinary truss can do, is to offer to the rails, or beams under them, a support, without fatiguing them by a compression at their ends and a pressure at their centre, as is the case with the ordinary way of bracing them, a way so singular that the more you screw the nuts at the ends of the rods, trying to make the beam stronger, the more you approach the final result, which will be to break it if you persist in making it stronger. When the elevated railroad, formed with my truss, has no train moving on it, there is not a particle of compression, or tension, or pression, or effort, of whatever nature on the rails or beams supported by the posts, and only when the train is moving is there, on two or three spans ahead of it, and only for a very short time, a very light tension or compression on the beams, considerably smaller than the one they would have if they were braced the ordinary way. This I am going to prove, and this reduction in the compression allows me to use material considerably lighter than with the ordinary way.

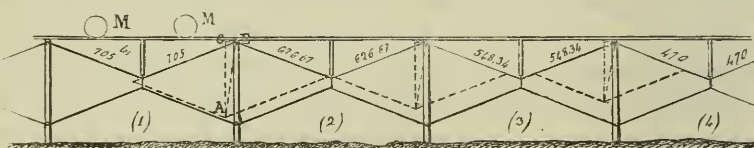
I have demonstrated with a model, at the meeting, that, if I form the truss with No. 23 wire, so that on a span 10 inches long and under a weight of 12 pounds suspended to both, or 6 pounds to each of them,

the top wires, two in number, have a deflection of 2·0625 inches, which gives each of them a tension of 8 pounds, and if, when once the truss is formed with the lower wires of same number and same deflection, I suspend again the weight 12 pounds to the truss, the tension on the top wires will only be the one, that a weight of 9 pounds suspended to each of them would give them and not a tension that a weight of 12 pounds suspended to each of them would give them, as the majority of people seem to think. The tension on each wire, which was 8, will become then 12 pounds, and not 16 pounds.

Let us suppose now the spans to be 10 feet and the deflection of the wire two feet, and that the truss on the different spans of the railroad is made with No. 6 wire, Trenton Iron Co. gauge, and that the moving load is a train formed of cars weighing 1,369 pounds each, a span of 10 feet giving room to two of these cars, and let us suppose the rail supported by the posts to be a 16-pound iron rail supported or not by beams. The centre of that rail is supported by the truss. On that part of the railroad where the train is not moving, there is not, as I said, a particle of tension or compression on that rail, while if it was braced the ordinary way, a thing which, by the way, would be difficult and costly to execute, that rail to support a weight of 1,396 pounds acting at its centre (3,369 pounds of car, and 27 pounds, half the weight of the rail 10 feet long), would have its two ends under a compression of 1,745 pounds, given by the bracing rods or wires fixed at its ends. Let us see now what will take place when the train is moving. First, let us state that in the parts where the train does not move, all the wires are under a tension of 470 pounds each, the temperature being supposed to be 45°F. Now when the first car of the train comes on a new span and when it has traveled the threee-quarters of it, the tension of the top wires in that span has increased gradually from 626·67 to 705 pounds, as I shall prove, and not from 470 to 705 pounds, suddenly, and the tension of the top wires in the two or three next spans ahead has also increased. The difference between 705 and 470 being 235, the tension on the next span will be, I suppose, 626·67 pounds; on the following span 548·34, and on the third span 470 pounds, the same as on the other spans ahead. On the railroad which I have built, full size, in Philadelphia, for experimenting, I found that it is so. That as the loaded car moves on it, the change in the tensions extends only a couple of spans ahead of the car.

It is a great mistake to think that if the tension of the top wires is

705 pounds in span (1) when the two cars M and M' are on it, the tension will stay at 470 pounds in span (2). I know the contrary, because I have seen that when the weight came on span (1), the beam of span (2) rose a little, and also that of span (3), although of a smaller quantity, and that in span (4) the beam did not rise, which shows, as I have said before, that the increase of tension communicates itself gradually to the wires of the spans ahead of the train, the effect being produced on only two or three spans the most. For example, when the cars will be in span (1), the tension of the top wires in span (3) will pass from 470 pounds to 548·33 pounds. Then when the cars will be in span (2), the tension of the wires in span (3) will pass from 548·34 pounds to 626·67 pounds, and finally, when the cars will be in span (3) the tension of the wires in span (3) will pass from 626·67 to 705 pounds. Now when on the bolt top of post B, the tension is one side 705 pounds, and 626·67 pounds on the other, the resultant of these two inequal tensions is in the direction AB and has an horizontal



component BC equal to 70 pounds; four times this, is 280 pounds. This would be all the compression that would take place on the end of the beam or rail in span (1) if, by a circumstance which will never happen, the span (2) and others ahead were going to vanish suddenly when the first car of the train is in position M'. The beams or rails will never be compressed with 280 pounds, because their ends are held by castings fixed on the posts. The posts will not move because they are braced solidly every fifty feet in the direction of the railroad, and all the way through by the rails or beams at their top, and as the effort is on the posts, if they have no motion, the rails or beams will not have any either, and consequently there will be no effort on them. There cannot be any compression without a tension on the next rail. The worst that may happen is that the effort of 280 pounds divides itself in two, or of 140 pounds compression on the end of rail in span (1), and the other of 140 pounds tension on the end of rail in span (2). Consequently I will never get more than 140 pounds compression on the top chord or rail of my truss, or *twelve and a half times less than if*

the rails were braced the ordinary way. This is one of the advantages of my truss, and I hope that those who could not see it, will see it now.

This would allow me, on a railroad built for a large traffic, in a trestle bridge with spans of 30 feet, made with two parallel trusses supporting each 1 ton of movable load per lineal foot, and braced in one case, the ordinary one, with iron rods $2\frac{1}{8}$ inches diameter, and in the other case, that is to say, with my truss, with cables $1\frac{5}{16}$ inches diameter, the deflection being $\frac{1}{8}$ of the span, to use for such spans only 2 beams $12'' \times 20''$ each instead of 4 beams $8'' \times 20''$, which would have to be used the ordinary way, on account of the compression, 33,600 pounds, under which each of those four beams would be if they were braced the ordinary way. I would have thus an economy of 1,600 pounds of timber costing $16 in one span only, which would allow me to build the bridge cheaper with my truss, and I call the attention of railroad engineers to this. I have made comparative estimates and know that it is so.

The economy with my truss is, per span of 30 feet, one ton and a half of beams, bolsters, and other materials, representing $45 in the cost of material, without counting the labor. Then my truss is stronger than the ordinary truss.

THE NEW BRITISH STANDARD WIRE GAUGE.

The makers of wire and sheet metals have, from time immemorial, designated the various sizes, diameters or thicknesses of their productions by a series of numbers; but the actual size represented by each of these numbers has had no universally accepted value. A multiplicity of so-called standards or gauges have been in existence, and endless confusion and trouble has been the result. Not only do the various standards vary, but different specimens of the same standard, even when made by the same maker, fail to agree together with the accuracy demanded by modern needs.

That which has undoubtedly the greatest currency is known as the Birmingham Wire Gauge, and can be traced back at least as far as the early part of the last century. It consists of a plate having in its edges a series of notches, each marked by a number indicating the thickness of the corresponding plate or wire. These notches have been added or altered as circumstances seemed to require, and together they form a series of sizes in some manner roughly proportionate to one another.

Many attempts have been made to harmonize the existing differences, so as to insure uniformity of size and a regular proportion—geometrical or empirical—between the various sizes.

To insure this uniformity of size, it has been repeatedly suggested that the whole difficulty could be obviated by expressing the sizes in thousandths of an inch. This was the conclusion reached by a Committee of the *American Institute of Mining Engineers,* whose "Report on a Standard Wire Gauge" was published in this JOURNAL, for February, 1878, and who recommended the micrometer screw caliper as the best known form of measuring device for this purpose. They did not, however, propose any series of sizes which would meet all requirements. For convenience, such a series of sizes must be agreed upon, and these sizes should bear some proportionate relation to one another: it is generally thought that the components of such a series could be most conveniently distinguished in trade by their numbers.

Many such series have been designed, but none of them have secured a universal recognition. Thus, the late Robert Briggs, C. E., proposed a gauge in which the reduction from one size to another should be 10 per cent. in diameter, or 19 per cent. in weight, starting with No. 0 = 1 centimetre. Mr. Latimer Clark proposed a series with the same starting-point, but with a reduction of 20 per cent. in weight and 10·557 per cent. in diameter.

In Great Britain the subject has long been discussed, and has of late received a new impetus from the manufacturers of electrical apparatus, to whom the size of the wire employed is often a vital matter. The feeling in England seems to have been that no gauge differing widely from the Birmingham gauge could be successfully introduced, and many attempts have been made to ascertain the exact sizes of the various numbers, and to modify them slightly, so as to bring the series of numbers up to modern requirements without entirely subverting present practice. This effort has finally culminated in the adoption by the *Standard* Department of the Board of Trade of Great Britain of a New Legal Standard Wire Gauge, which fixes a definite value for each number of the Birmingham Gauge.

Through the courtesy of Prof. J. E. Hilgard, Superintendent of the U. S. Coast and Geodetic Survey, we are enabled to present herewith official tables giving the New Standards in millimetres and in parts of an inch. Professor Hilgard has also favored us with a series of interesting analyses of the new and old gauges, which we are glad to have an opportunity of putting on record.

NEW LEGAL STANDARD WIRE GAUGE.

Descriptive Number. B. W. G.	Equivalent in parts of an inch.	Descriptive Number. B. W. G.	Equivalent in parts of an inch.
No. 7/0	0·500	No. 23	0·024
6/0	464	24	22
5/0	432	25	20
4/0	400	26	18
3/0	372	27	0164
2/0	348	28	148
0	324	29	136
1	300	30	124
2	276	31	116
3	252	32	108
4	232	33	100
5	212	34	0092
6	192	35	84
7	176	36	76
8	160	37	68
9	144	38	60
10	128	39	52
11	116	40	48
12	104	41	44
13	92	42	40
14	80	43	36
15	72	44	32
16	64	45	28
17	56	46	24
18	48	47	20
19	40	48	16
20	36	49	12
21	32	50	0010
22	28		

11,367.—500.—2|84. Wt. 2,3838. E. & S.

NEW LEGAL STANDARD WIRE GAUGE.

Descriptive Number. B. W. G.	Metric equivalent in millimetres.	Descriptive Number. B. W. G.	Metric equivalent in millimetres.
	m.m.		m.m.
No. 7/0	12·700	No. 23	0·610
6/0	11·785	24	0·559
5/0	10·973	25	0·508
4/0	10·160	26	0·457
3/0	9·449	27	0·4166
2/0	8·839	28	0·3759
0	8·229	29	0·3454
1	7·620	30	0·3150
2	7·010	31	0·2946
3	6·401	32	0·2743
4	5·893	33	0·2540
5	5·385	34	0·2337
6	4·877	35	0·2134
7	4·470	36	0·1930
8	4·064	37	0·1727
9	3·658	38	0·1524
10	3·251	39	0·1321
11	2·946	40	0·1219
12	2·642	41	0·1118
13	2·337	42	0·1016
14	2·032	43	0·0914
15	1·829	44	0·0813
16	1·626	45	0·0711
17	1·422	46	0·0610
18	1·219	47	0·0508
19	1·016	48	0·0406
20	0·914	49	0·0305
21	0·813	50	0·0254
22	0·711		

The table below presents the values that have been adopted by Act of Parliament for the new British Legal Standard Wire Gauge.

Descriptive Number. B. W. G.	Equivalent in parts of an inch.	Differences.	Descriptive Number. B. W. G.	Equivalent in parts of an inch.	Differences.
No. 7/0	0·500	0·036	No. 23	0·024	0·002
6/0	464	32	24	22	2
5/0	432	32	25	20	2
4/0	400	28	26	18	16
3/0	372	24	27	164	16
2/0	348	24	28	148	12
0	324	24	29	136	12
1	300	24	30	124	8
2	276	24	31	116	8
3	252	20	32	108	8
4	232	20	33	100	8
5	212	20	34	92	8
6	192	16	35	84	8
7	176	16	36	76	8
8	160	16	37	68	8
9	144	16	38	60	8
10	128	12	39	52	4
11	116	12	40	48	4
12	104	12	41	44	4
13	92	12	42	40	4
14	80	08	43	36	4
15	72	8	44	32	4
16	64	8	45	28	4
17	56	8	46	24	4
18	48	8	47	20	4
19	40	4	48	16	4
20	36	4	49	12	2
21	32	4	50	10	
22	28	4			

It will appear at first glance that this is the Birmingham or Stubs' gauge, considerably simplified. In that the numbers representing the different sizes are in a majority of cases prime to each other, and simple exact ratios impossible. In the new gauge all the numbers, as given in the table, with three exceptions, are multiples of 4, and it is

possible for one gauge to be exactly double some other. This occurs in nineteen cases, beginning with Nos. 6|0 and 4 and ending with Nos. 47 and 50. There is, of course, an approximation to this ratio in many other cases, but the limitations of the problem seem to prevent a greater number of exact agreements.

The figures in the third column of the table show how much less each following number is than the preceding. Here also there is a progression by 4s (with the exceptions noted above), though the number of times that each number enters is governed by no law, 36 entering once, and 8 fourteen times.

Descriptive No.	New Leg. Std.	Birmingham.	D. B. & S.	Descriptive No.	New Leg. Std.	Birmingham.	D. B. & S.
0000000	0·500			23	0·024	0·025	0·22571
000000	464			24	22	22	201
00000	432			25	20	20	179
0000	400	0·454	0·46	26	18	18	1594
000	372	425	40964	27	164	16	14195
00	348	380	3648	28	148	14	12641
0	324	340	32495	29	136	13	11257
1	300	300	2893	30	124	12	10025
2	276	284	25763	31	116	10	8928
3	252	259	22942	32	108	9	795
4	232	238	20431	33	100	8	708
5	212	220	18194	34	92	7	6304
6	192	203	16202	35	84	5	5614
7	176	180	14428	36	76	4	5
8	160	165	12849	37	68	....	4453
9	144	148	11443	38	60	....	3965
10	128	134	10189	39	52	....	3531
11	116	120	90742	40	48	....	3144
12	104	109	80808	41	44		
13	92	95	71961	42	40		
14	80	83	64084	43	36		
15	72	72	57068	44	32		
16	64	65	5082	45	28		
17	56	58	45257	46	24		
18	48	49	40303	47	20		
19	40	42	3539	48	16		
20	36	35	31961	49	12		
21	32	32	28462	50	10		
22	28	28	25347				

Above are placed, side by side, the new scale, the Birmingham* scale and the Darling, Brown & Sharpe scale, the values being given in fractions of an inch.

By comparison of the first two scales it will be seen that for the old series of four sizes from 0000 to 0 is substituted a series of seven sizes from 0000000 to 0, the first lying entirely outside the old series, and being exactly $\frac{1}{2}$ inch. At No. 1 there is agreement between the two scales, the value being 0·3 inch. From this point to No. 30 there is substantial agreement. The six sizes following No. 30 in the old series range from 0·1 to 0·004. To these correspond twelve sizes in the new. The last eight numbers in the new series have no counterpart in the old.

Influence of High Pressures on Living Organisms—The recent expedition of the *Talisman* has furnished interesting proofs of the existence of organic life at great depths in the sea. Direct experiments have been made by M. Regnard, with the press of Messrs. Cailletet and Ducretet, by which a pressure of more than a thousand atmospheres can be obtained, corresponding to a depth of water of more than 10 kilometres (6,214 miles). Beer-yeast, when submitted to the pressure of a thousand atmospheres for an hour, and then placed in contact with sweetened water, appeared to be latent. After about an hour it revived, when fermentation began and went on slowly. Yeast was afterwards left in sweetened water, under a pressure of 696 atmospheres for seven hours, when no fermentation took place. The tube was then withdrawn and, after an hour, fermentation began. Under the pressure of a thousand atmospheres, starch was transformed into sugar by saliva. Algæ, when submitted to six hundred atmospheres, for an hour, were able to decompose carbonic acid under solar influence; but four days afterwards they were dead and began to putrefy. Seeds of cress, after ten minutes exposure under a thousand atmospheres, were swollen with water, and it was a week before they began to sprout. Stagnant water, swarming with infusoria, was subjected to six hundred atmospheres. After a half-hour's exposure the animals seemed to be asleep, but after they were withdrawn, they soon revived. Molusca, bloodsuckers, and crustacea, though apparently asleep or dead under the pressure, revived more or less rapidly after being withdrawn. Fishes, without a swimming-bladder, can be subjected with impunity to the pressure of a hundred atmospheres. At two hundred atmospheres they seem to be asleep but may be quickly revived. At three hundred atmospheres they die. At four hundred atmospheres or more they are dead and rigid; they putrify even without losing their rigidity.—*Chron. Industr.*, April 6, 1884. C.

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* The values for the Birmingham scale here given are those which were determined in 1846 by Mr. Holtzapffel, who examined the best specimens of the gauge then obtainable. These values have been widely adopted, although they differ more or less from the figures given by other and more recent authorities.

REPORT ON THE TRIAL OF THE "CITY OF FALL RIVER."

By J. E. SAGUE, M. E., and J. B. ADGER, M. E., with an introduction by Professor R. H. THURSTON.

(Continued from Vol. cxviii., page 74.)

The reaction of the stream of water acted upon by any propelling instrument, equals the product of the mass of a cubic foot of water, the number of cubic feet of water acted upon in a second, and the velocity in feet per second impressed upon that water by the propeller. This reaction equals the total forward thrust on the paddle shaft. In the case of a propeller working in a current previously induced by the ship in its own direction, technically known as the following current, and owing to "skin" friction or otherwise, there is a loss of thrust owing to the increased flow caused by the propeller; and a mathematical investigation proves this to be always a source of waste power.

The following current though affecting greatly the working of screws placed at the stern, probably does not act to any appreciable extent upon a paddle placed as far forward as the one under consideration; and, as there is also no way of determining the velocity of such a current beyond a rough approximation, it is neglected here, as well as by Rankine in his examples from paddle steamers. Letting $T =$ total thrust $=$ effective thrust in case of no forward current.

$u =$ velocity of ship $=$ velocity of water relatively to ship in the absence of the propeller.

$v =$ actual backward velocity of water with reference to ship.

$a =$ area of stream driven back by propeller.

$\frac{w}{g} =$ mass of 1 cubic foot of sea-water $= 2$ nearly.

Volume of water acted upon $= v \times a$;

Velocity impressed by propeller $= v - u$;

$$T = \frac{w}{g} v(v - u)a.$$

To apply this general formula to feathering wheels let $r' =$ radius to centre of pressure. $n =$ revolutions per second. $2\pi nr' =$ velocities of centres of pressure of floats relatively to ship, which $= v$; as with feathering floats the velocity of stream driven aft $=$ velocity of floats

themselves. Let $2\ \pi n r_0 =$ velocity of ship, then r_0 is the radius of apparent rolling circle; and since we have neglected the forward current, it is also the radius of the real rolling circle.

$2\pi n(r' - r_0) =$ slip of paddles $= (v - u)$.

Then $T = \frac{w}{g} 4\pi^2 n^2 r'^2 . \frac{r' - r_0}{r'}$ which, in this case, should equal the resistance of the vessel.

Useful work per second $= 2\pi n T r_0 =$ resistance $\times$ speed of ship.

Total work $= 2\pi n r' T =$ resistance or thrust $\times$ speed of paddle.

Efficiency $= \frac{r_0}{r'} = 1 -$ slip.

In applying these formulæ to the *City of Fall River*, we have taken the same data as that used in calculating the probable speed for the two complete trips of May 3 and 4, and May 9 and 10. The mean centre of pressure was found from the following formula deduced mathematically by Van Buren.*

$$x = R_2 \frac{3(1-p^4) - 8(1-p^3)\, m \cos.\varphi + 6(1-p^2)\, m^2 \cos.^2\varphi}{4(1-p^3) - 12(1-p^2)\, m \cos.\varphi + 12(1-p)\, m^2 \cos.^2\varphi}$$

in which R_2 and $R_1 =$ outer and inner radii to bucket, $\varphi =$ angle of bucket with vertical, $p = \frac{R_1}{R_2}$; m=ratio of velocity of vessel to velocity of outer circumference of wheel. $\cos.\varphi = 1$, $R_2 = 12{\cdot}75'$, $R_1 = 9{\cdot}416,'$ $p = {\cdot}7385$ $m = {\cdot}7$. Substituting these gives for x or the distance from centre of wheel to centre of pressure, $11{\cdot}22'$. Hence $x = r' = 11{\cdot}22'$.

	First Trip.	Second Trip.
$n =$	·4286	·427
$\frac{r' - r_0}{r'} =$	·192	·204
Resistance = ...	23721	24836
Thrust = ...	23589	24772
Difference = ...	132	64
Efficiency = ...	80·8 per cent.	79·6 per cent.

	First Trip.	Second Trip.
$2\pi T n r' =$	709,740 ft. lbs. per second.	745,690 ft. lbs. per sec.

which are the energies in foot-pounds per second expended in driving

* JOURNAL OF FRANKLIN INSTITUTE, Vol. 49, 1865.

paddles, exclusive of friction. Dividing by 550 we have for the corresponding horse-powers exerted effectively in driving paddles:

	First Trip.	Second Trip.
Horse-powers	1290	1355
Indicated horse-power,	1595	1619
Horse-power expended in overcoming friction,	305	264

The mean efficiency of the engine and mechanism of the wheel is given by the quotient of mean effective horse-power, divided by mean indicated horse-power, which is $\frac{1322 \cdot 5}{1607} = 82 \cdot 3$ per cent.

The resultant efficiency of engines and paddles is the product of the mean efficiency of each, or 80·2 per cent. $\times$ 82·3 per cent. = 66 per cent.

The resistance of the vessel as given above was computed from Rankine's formula, using a coefficient diminished in the ratio of the coefficients of propulsion, or 23,500 to 20,000; the latter of which was determined by Rankine from experiments on iron ships, and the former by Messrs. Elliott and Lieb from experiments on copper-bottomed vessels of similar form to the one under consideration.

This reduced formula is

$$R = \frac{\text{Augmented surface} \times \text{square of speed in knots.}}{117 \cdot 5}$$

In Rankine's formula, given in his "Ship-building," the constant as determined for iron ships is 100.

The differences above between the resistances and thrusts, being 132 in the first case and 64 in the second, must be owing somewhat to errors of assumption, such as neglecting the following current, and also to approximate computations. In the paddle efficiency here given, no loss is considered but that due to slip.

The additional losses are those due to the small amount of oblique action and to friction. The first, though undoubtedly slight, must still be more in a wheel of 4 feet 9 inches dip, as in the present case, than in one having the buckets simply immersed; but owing to the complexity of the action in a wheel of this sort, due to dip and varying angles, as well as varying centre of equal radial wheel, it is impossible to determine with any degree of certainty the losses due to this cause. Friction is greater here than with a radial wheel,

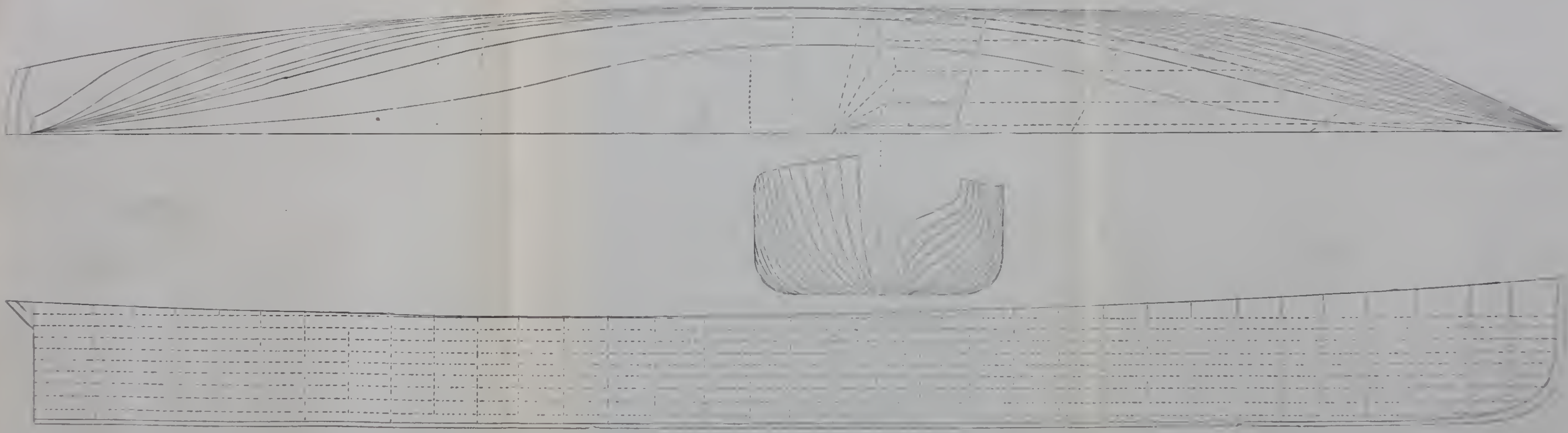


FIG. 1. LINES OF STEAMER "CITY OF FALL RIVER."

embracing both fluid and journal friction, each of which is indeterminate by analysis.

DESCRIPTION OF ENGINE.

The engine of the *City of Fall River* was built by Messrs. W. & A. Fletcher, of New York, from the designs of their superintendent, Mr. Stevenson Taylor. It is a vertical "beam engine," compounded and fitted with a surface condenser, and capable of indicating 2,000 horse-power. It is similar in general appearance to the ordinary American beam engine, but has two cylinders,—a high and a low pressure,—both connected to the forward end of the beam.

The piston of the low pressure cylinder is connected to the end of the beam by a pair of links journaled on a cross-head sliding in vertical guides, which are bolted to the cylinder head and supported by stays from the "gallows frame." The high-pressure cylinder is placed just abaft the low-pressure, and connected to the beam in the same manner, one-third the distance from the forward end to the centre. The connecting-rod, placed at the after extremity of the beam, actuates the paddle-shaft, while the air-pump plunger is driven by a pair of links, journaled on the same side of the beam at an intermediate point. There is a single acting feed-pump for each of the two boilers, the plungers of which are driven directly from the air-pump cross-head. The gallows-frame is of wood and of the ordinary design. It is composed of struts diverging from the beam centre pillow-blocks and strongly framed together by longitudinal pieces and knees, and resting upon horizontal beams beneath the engine bed-plate. The paddle-shaft pillow-blocks are placed between two of the main struts near their lower ends, the shaft itself revolving beneath the main deck.

The steam pipes from the boilers unite near the high-pressure steam chest, which takes steam at the lower end and distributes it through side pipes to the upper valves. The receiver is simply an enlarged steam pipe taking the exhaust from the high-pressure cylinder and leading it to the bottom of the low-pressure chest, where it is carried through side pipes as before.

The steam pipes are so arranged that the steam can be led directly into the receiver by introducing a pipe which lies at hand. The high-pressure cylinder can then be disconnected by closing its throttle and stopping up its exhaust pipe. The live steam from the boiler is thus carried directly into the low-pressure cylinder, and by disconnecting

the high-pressure piston rod the engine is converted completely into the simple type of an ordinary American beam engine. The steam is distributed by double poppet valves and the Stevens gear; the steam and exhaust valves being actuated by independent eccentrics through a system of cams or "wipers" placed upon horizontal rock-shafts, and "toes" fastened upon the valve rods. The cut-off is fixed and the valves are adjusted, as usual with this form of gear, to close at about half stroke in each engine. The condenser is placed beneath the cylinders, and is rectangular in shape. The condensing water is forced through tinned copper tubes, around which the exhaust steam flows. A centrifugal pump of a capacity of about 5,000 gallons per minute is used as a circulator, and is driven by a small vertical engine taking steam from the main boilers.

The steam chests and side pipes all have a coating, half an inch in thickness of mineral wool, outside of which is wood-lagging, and the whole covered with a thickness of Russian iron. The cylinders are covered partly with a layer of mineral wool and partly with felting, outside of which is a layer of wood-lagging put on in the usual manner. The steam pipes are all felted. Neither cylinder is steam-jacketed. The principal dimensions of the engines are as follows:

Distance of top of engine frame above engine keelsons	39	feet	10	inches.
Length of beam between centres	26	"		
Depth of beam outside to outside of strap	12	"	6	"
Length of connecting rod	28	"	9	"
Diameter at centre*			12½	"
Diameter at ends			8	"
Diameter of paddle-shaft in main journals			16½	"
Diameter of crank pin			9½	"
Length of crank pin journal			12	"
Length of each beam centre journal			17	"
Diameter of beam centre journal			11	"
Diameter of main steam pipes			14	"
Diameter of air-pump			37	"
Stroke of air-pump	4	"	9	"
Diameter of feed-pumps			5⅜	"
Stroke of feed-pumps			57	"
Length of L. P. links from centre to centre	9	"		
Length of H. P. links from centre to centre	15	"	2	"

* Made heavy, so that rod with air-pump will balance extra weight of high pressure piston, cross-head and links.

Volume of receiver	89·13 cubic feet.
Number of condenser tubes	2,431
Length of each	9 feet.
Diameter of each	$\frac{3}{4}$ inches.
Amount of condensing surface	4,057 square feet.
High-pressure cylinder, diameter	44 inches.
High-pressure cylinder, stroke	8 feet.
Net area of each steam port in percentage of piston area	10·7 per cent.
Same for each exhaust port	11·02 "
Total volume in clearance and steam passage at top. Same for bottom, average	3·845 cubic feet.
Average clearance at each end in percentage of stroke volume	4·6 per cent.
Total length in clearance	$2\frac{11}{16}$ inches.
Diameter of piston rod	$6\frac{1}{2}$ "
Low pressure cylinder, diameter	68 "
Low pressure cylinder, stroke	12 feet.
Net area of each steam port in percentage of piston area	7·1 per cent.
Same for each exhaust port	7·3 "
Total volume in clearance and steam passage at top. Same for bottom, average	9·18 cubic feet.
Average clearance at each end in percentage of stroke volume	3·05 per cent.
Total length in clearance	2 inches.
Diameter of piston rod	$7\frac{1}{4}$ inches.

BOILERS.

There are two return tubular boilers, one set forward and the other abaft the engine, directly over the keel. They are of the Redfield tubular pattern, each having two shells made of half-inch Otis steel plates, double riveted on longitudinal seams, and calculated to carry a working pressure of one hundred pounds per square inch. Each boiler is provided with a superheater surrounding the uptake, which, however, is not designed to give any considerable degree of superheating, but merely to dry the steam. The steam pipes are carried from this chamber. Each boiler has two separate furnaces, and four fire-doors. Natural draft alone is used.

The principal figures are as follows:

Total height to top of superheater	$24\frac{1}{2}$ feet.
Length of each boiler	15 "
Width of each boiler	$17\frac{1}{2}$ "
Diameter of each shell	$7\frac{1}{2}$ "

Number of tubes in each shell	110
Length of each tube	12 "
Diameter of each tube	3½ inches.
Diameter of uptake	56 "
Diameter of superheater	96 "
Height of superheater	12 feet.
Area of grate surface in each boiler	115 sq. feet.
Area of water heating surface in each boiler.	3,345 "
Area of steam heating surface in each boiler.	205 "
Ratio of water heating to grate surface..	29
Ratio of total heating to grate surface..	30·87
Weight of each boiler	51½ tons (net)
Weight of water in each boiler..	27 " "
At 12 inches above tubes	

MANNER OF MAKING TESTS.

The experiments upon the Steamer *City of Fall River*, for the purpose of ascertaining the economy of her engines and boilers were made during the regular trips of the vessel between the cities of New York and Fall River, and in each case were performed on both the outward and return passages, the time occupied in each, being from ten to twelve hours, the runs being make during the night. The times chosen for making the trials were such as simply suited our own convenience and the conditions were all those of ordinary running, not the slightest change or adjustment having been made by either the engineers or the builders to secure high results, and those obtained are reliable indications of the daily performance. All the observations and measurements were made that are necessary to a satisfactory test of both the engines and boilers, and all were made by persons accustomed to the duty and acquainted with the objects in view and interested in the thorough performance of the work.

The principal measurements were checked as far as possible by having them made independently, and the agreement of the results obtained upon successive trips proves conclusively the accuracy of the work. The water measurements, being those upon which the success of the test most largely depended, need a somewhat complete description. Owing to the great expense necessary to fit up tanks to measure the water in the ordinary manner, as well as the difficulties arising from handling the great quantities used while the vessel is in free route, it was decided to use meters, and the Worthington piston meter was adopted as the one most certain of giving accurate results under the

prescribed conditions. Two three-inch meters each capable of measuring 18 cubic feet of water per minute, were furnished for the test by the Worthington Hydraulic Works of Brooklyn, who also furnished the plans for the meter connections, and suggested the manner of testing them. A meter *A* was placed as near as possible to each boiler,

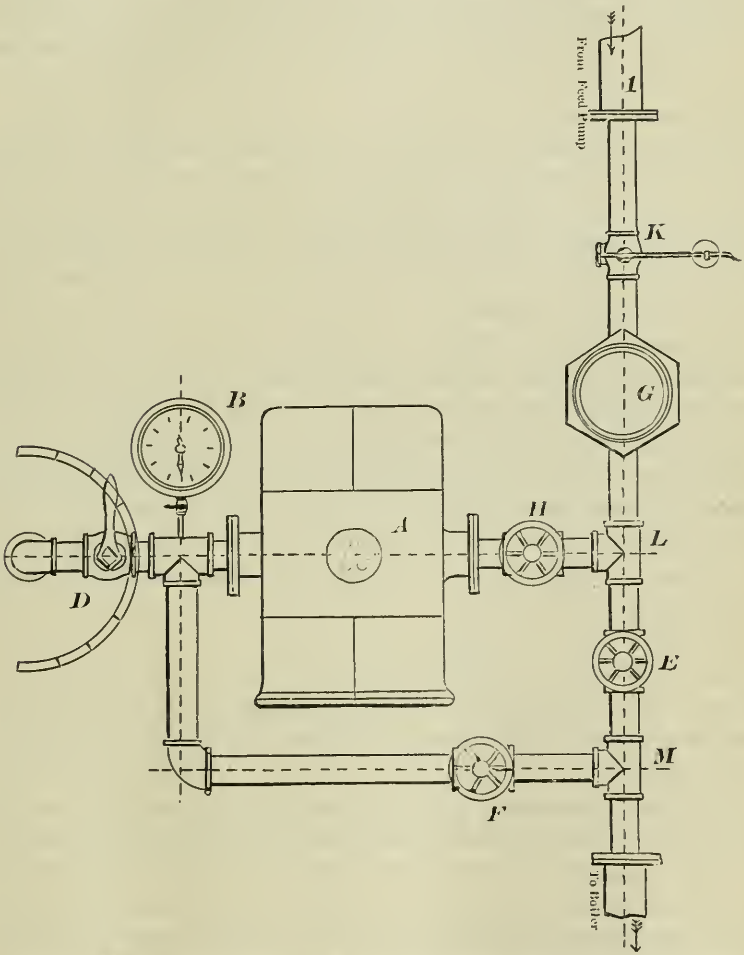


FIG. 2.—Meter connections. $2\frac{1}{2}$ inch pipe throughout. .3 inch meter.

and introduced by taking out a piece of the feed-pipe, (*I*). On the line of this joint was placed a bye-pass, (*G E*) from which connections were made by "*Ts*" (*L M*), to pipes leading to the meter, (*A*) which

rested upon a platform as near as possible to the main feed-pipes, (I). A globe valve (H & F) was placed in each pipe so that the water could be shut off from the meter when the latter was not needed, and allowed to go direct to the boiler. The feed-pumps being single acting and connected to the beam tend to cause a violent shock at each stroke of the engine, and for the purpose of lessening this and to avoid injury to the meters, a large air chamber (G), was placed in each pipe, which, with one already there, reduced the pulsating action to a very small amount. A safety valve (K) and pressure gauge (B), were also placed upon the line of connections.

In order to obtain a factor for a cubic foot reading on the meter dial, a method of testing was adopted which had been used in pumping engine tests by the Worthington Hydraulic Company, and found to give results which agreed with a tank measurement to within one-tenth of one per cent. A test was made each hour during the whole time of the trials, as follows:

The pressure-gauge, which was placed just behind the meter, was first observed, and the pressure and amount of pulsation noted, the latter being between five and ten pounds. Then the globe-valve leading to the boiler was closed, and a plug-cock, placed over a cask, opened, the water being allowed to flow through into the cask, which was placed upon a platform scale, until the dial finger moved. This movement, in the Worthington meter, takes place only at the end of a certain number of strokes, and after a little practice the flow can be stopped at any even reading very accurately. After the flow had thus been interrupted, the scales were balanced and the plug-cock again opened and adjusted until the throttling had produced as near as possible the same pressure and vibration as at the preliminary reading. Four cubic feet were then allowed to flow through into the barrel, the dial registering this quantity with great exactness. The plug-cock was then closed quickly, just at the end of the movement of the pointer, and the valve opened to the boiler, the four cubic feet being carefully weighed and the whole quantity of water used in the meter test noted, and subsequently subtracted from the dial readings. At the time of each meter test the temperature of the feed-water was noted in the barrel, after which it was emptied into the bilge.

The results of the ten or twelve meter tests thus secured each night under the exact conditions of use, do not differ in any important degree from each other, and the average being taken, permit the calculation

of the water consumption with as great a degree of accuracy as could be reached by any tank measurement. The dial readings and water-levels at each boiler were noted simultaneously at hourly intervals, allowing the calculation of the engine economy to be made between any required points.

All the coal was weighed in boxes upon platform scales, the boxes being filled up to the same weight each time, and two independent records being kept of them, which were compared at intervals and found to agree throughout.

Indicator cards were taken every half hour from each end of both cylinders, by the best Thompson and Tabor instruments, and observations were made at the same time in the engine room, of the pressure in the steam and receiver guages, reading of barometer and thermometer and revolution counter. Observations were also made, at regular intervals, of the temperatures in the chimneys, and of the temperature of the entrance and exit water from the condenser. At the moment of passing each light-house a bell was rung in the engine room by the pilot, the time of which was noted and entered with the reading of the revolution counter, opposite the name of the light-house on the log. The wind and tide were also noted at intervals. The Sickles Steam Steering Gear is used, and takes steam from the forward boiler, but owing to the straight and long courses run, the work required of it was slight, and the quantity of steam used inappreciable compared with that taken by the main engines.

CALORIMETER TESTS.

The quality of the steam was determined by means of a steam calorimeter, in which the steam was condensed in a copper coil. The weights were obtained on a steelyard graduated to quarter ounces, and the temperatures noted on a thermometer graduated to half degrees. Each of these half degrees occupies a little more than $\frac{1}{8}$ inch on the scale, so that the temperature can be read accurately to within one quarter degree.

The main steam pipe of the *City of Fall River* was tapped just in front of the high pressure steam chest and a half-inch pipe, upon which a long thread had been cut was inserted, until it extended several inches up into the steam pipe, and drew its supply from near its centre. From this half-inch pipe a steam hose conveyed the steam to the calori-

meter, and on this pipe were a steam gauge and a steam thermometer, on which the pressure and the temperature of the steam could be observed. All the observations were made by men accustomed to the use of this calorimeter and were as accurate as could be obtained.

Each test was entered upon a log previously prepared for the purpose, having columns for time, initial and final weights of the water in the calorimeter, initial and final temperatures of the water in the calorimeter, steam gauge pressure and the temperature from the steam thermometer.

In calculating the results, an allowance has been made for the absorption of heat by the calorimeter itself; this amount having been previously determined accurately for the range over which the calorimeter was used. Also, as the water used was for convenience seawater, an allowance has been made for the specific heat of the salt contained therein. $\frac{1}{32}$ was taken as the saltness of the water and ·555 as the specific heat of the salt in the fluid condition. This allowance was found to make 70 pounds of seawater equivalent to 68·786 pounds of fresh water, and this figure was used in the calculations.

On the night of May 9th, nine tests were made, giving an average of 1.65 per cent. priming. The range of the nine figures obtained was from + 5·4 per cent. to — 1·49 per cent.; two of the nine being negative, or showing a slight degree of superheating.

On the night of May 10th, seven tests gave an average of 0·67 per cent. priming. The range was from + 3·67 to — 4·76. Three of the seven showed superheating.

BOILER TESTS.

In making a boiler test to ascertain its evaporative power, it is necessary to start the test with a certain level of water in the boiler and a certain pressure of steam registered upon the gauge. At the close of the test the water should stand at the original level and the steam pressure falling, *i. e.*, the fire pretty well burnt out. Between the beginning and end of the test every pound of coal and ash should be carefully weighed, and all the water supplied to the boiler accurately measured. It is customary to raise the steam up to the gauge pressure at which the test is to be made, with a wood fire; to then draw this fire, and make a new one as quickly as possible with weighed fuel, and begin the test from the lighting of this second fire. Towards

the end of the test the fire is allowed to cool down, and the water is pumped up to the original level, or the difference of level is noted, and the remains of the fire are drawn out and weighed as ash.

It should be kept prominently before the mind that the tests of the *City of Fall River* were made during the regular trips of the boat without any special previous preparation, and, therefore, the impracticability of fulfilling the above named conditions as to starting rendered an exact boiler test impossible.

An approximate figure, however, and one very closely approximate can be obtained upon the following basis: Upon the vessels reaching port each morning, the fires are cleaned and banked, and to this bank coal is added about noon. About two hours before starting in the afternoon the banks are pulled down and spread, and coal is thrown upon the fires. The steam made is allowed to blow off through the escape valves until the hour for leaving approaches. The valves are then closed and the pressure allowed to run up to about 65 pounds. By ascertaining the level of water in the boilers at the time of spreading the fires, and also its temperature, we can get the number of heat units required to raise this amount of water up to the temperature corresponding to the boiler pressure at the start, and we can find the number of pounds of water which this number of heat units could have evaporated. Thus we can account for that part of the coal used in raising the temperature of the water in the boiler. But the whole boiler, weighing in this case 51·5 tons, is heated up and the rise of temperature varies considerably for the different parts. What this rise is, cannot be exactly stated, but it is deemed fair to assume that the whole boiler is heated from the temperature of the water in it at the time of "pulling the banks" to the temperature corresponding to the steam pressure when the engine starts. The whole boiler is therefore treated as a mass of iron and by multiplying this weight by the specific heat of iron and by the rise of temperature, we can get the number of heat units absorbed in heating up the boiler. Another source of perplexity is the bank, consisting in each case of 2,000 or 3,000 pounds of coal, consumed partly, but how much it is impossible to state. The assumption here made was that the banks balanced and that the coal thrown on at noon was merely equivalent to the consumption of the bank during the day, and on this assumption every pound of coal put on the fires, from the time of "pulling the banks"

until the fires is banked the next morning, in port, is charged to the boilers. This assumption is at least safe as the error in it is certainly against the boiler. Upon these assumptions the following table is made out. The coal for the three nights was carefully weighed and the temperature of the water at the time of "pulling the banks" and the various water levels on each occasion were noted and entered upon the logs. The exact widths of the boilers at various levels were obtained from the drawings in the possession of the builders, and the different weights of water at the different levels were very carefully calculated. The specific heat of iron was taken as ·1138. The weight of the boiler multiplied by this figure and by the rise of temperature gives the number of thermal units absorbed, and this number divided by the difference between the total number of thermal units contained in the steam at the boiler pressure and the temperature of the feed, gives the number of pounds of water, which the heat expended in raising the temperature of the boiler could have evaporated. This amount is added to the total number of pounds evaporated by the boiler. For example, on the night of May 10th, the temperature of the feed was 97·16° Fahr.; the total number of heat units of one pound of steam at the boiler pressure was 1209·56; the difference between the two numbers is 1112·4. We then have 103,000 pounds, the weight of the boiler, multiplied by ·1138, the specific heat of iron, and by 130°, the range over which the temperature was raised, equal to 1,523,782. This number divided by 1112·4 gives 1,369 pounds of water, as the number which the heat absorbed in heating up the boiler might have evaporated, and this amount is added to the total number of pounds with which the boiler is credited.

The same process is pursued in all the tests. The difference, seen below in the percentage of ash, is due no doubt to difference in firing forward and aft.

RESULTS OF BOILER TRIAL.

Position of Boiler.	For'd.	Aft.	For'd.	Aft.	For'd.	Aft.
Date of test.	May 4	May 4	May 9	May 9	May 10	May 10
Length of test (hours)	13	13	12·5	12·5	12·5	12·5
Total coal burned (lbs.)	18922	21314	18294	22083	18186	21650
Total refuse, ash, etc. (lbs.)	3699	3427	3518	3206	3449	3503
Total combustible (lbs.)	15223	17887	14776	18877	14737	18147
Percentage of refuse, ash, etc	19·55	16·08	19·23	14·52	18·97	16·18
Total water evaporated (lbs.)	154106	183785	151057	182019	144391	176576
Average Steam Guage Pressure	68·5	68·5	70	70	70.	70
Average height of barometer	30·70	30·70	30·70	30·70	30·5	30·5
Average temperature of feed water (Fahr.)	102·°3	101°	97·1°	97°	97°	97·3°
Average temperature of atmosphere (Fhr.)	74°·	74·°	78°	78°	77°	77°
Average temperature of chimney gases (Fahr.)	435°	485°	485°	495°	493°	416°
Number of pounds of coal per hour per sq. ft. of grate	12·657	14·257	12·726	15·292	12·646	15·069
Number of pounds of water evaporated from the temperature of feed per lb. of coal	8·14	8·62	8·257	8·24	7·939	8·156
Number of pounds of water evaporated from the temperature of feed per lb. of combustible	10·117	10·27	10·22	9·639	9·797	9·732
Number of pounds of water evaporated from and at 212° F. per lb. of combustible	11·59	11·77	11·75	11·08	11·266	11·192

(To be concluded.)

ATMOSPHERIC CHANGES AT NICE.—Since the appearance of the brilliant sunsets, Messrs. Thollon and Perrotin have noticed that the sky at Nice seems to have lost much of its ordinary transparency. They have been accustomed, on every fair day, to examine the sky in the neighborhood of the sun, placing themselves near the border of the shadow projected by one of the observatory buildings. When thus sheltered from the direct rays of the sun, they have noticed in former years that the blue of the sky continued to the very borders of the solar disk. If they were so placed that the disk was almost a tangent to the border of the screen, but still invisible, no increase in the brilliancy of illumination indicated the place where the point of tangency would be found. This is now no longer the case. Since the month of November, even upon the brightest days, the sun appears constantly surrounded by a circular fringe of dazzling white light, slightly tinged with red at its outer edge, and with blue on the inner edge. There is a sort of ill-defined coronna, with an apparent radius of about fifteen degrees. It would be interesting to know whether this fact is general and whether it can be considered as connected with the volcanic dust or other causes of the late brilliant twilights.—*Chron. Industr.*, April 6, 1884.

TESTS BY HYDROSTATIC PRESSURE.

By S. Lloyd Wiegand, M. E.

[Read at the Stated Meeting, Wednesday, June 18, 1884.]

In the course of some experiments, made for testing the strength of vessels to resist internal pressure, some important facts appeared, which although not directly relevant to the immediate purpose of the inquiry, appeared interesting and important to several acquaintances engaged practically in similar operations, and upon an interchange of experiences and opinions with them, the writer found that these facts were either not generally known, or else their importance had been frequently overlooked where they should have been considered and attentively regarded.

The matters referred to are the performances of the apparatus used for testing by hydraulic or hydrostatic pressure, and the inaccuracies of pressure recording gauges.

In the matter of applying hydrostatic pressure, the momentum of the fluid as forced into the vessel plays an important part, and sometimes, if not frequently, by concussion, causes ruptures or leaks in vessels at pressures far below those that they have resisted under slow and steady application of hydraulic force.

When force pumps of large displacement are used, the shock of fluid-column delivered at each stroke is very appreciable in producing a pulsation or intermittent bulging or dilation of flat surfaces, and leakage and rupture occurs at a moment coincident with or instantly following the sudden injection of the fluid, and the indices of the pressure gauges jump to an indication of pressure far beyond that which they settle at. This is specially noticeable in what is known as maximum recording gauges, the rising and returning motions of the index to which motion from the pressure is first imparted being often so sudden as to escape observation, and the recording index may be seen standing considerably beyond the point to which the eye can follow the moving index.

When a pump with a smaller plunger is used, the pulsation of the flat parts of the vessel is much less appreciable, and oscillations of the gauge indices are proportionally reduced, whilst at the same time the pressures reached before leakage and rupture are greatly increased.

These differences in performance under different proportions of apparatus are so well understood by the makers of machines for testing the tensile strength and properties of metals by weighing beams combined with a hydraulic jack or press, that they employ a series of very small reciprocating force pumps so worked by cams or cranks from a revolving shaft as to effect an almost uniform delivery of fluid to the ram of the jack, and their experience shows results not procurable by intermittently or concussively acting pumps; pressure produced by the use of an injector produces like results.

The steady or more uniform application of pressure has sometimes been accomplished by charging an accumulator, similar to those used for hydraulic elevators and cranes, with fluid under a pressure equal to the intended test, and then cautiously admitting the fluid to the vessel under test. The cost and want of portability in this apparatus, as well as the risk of accident from sudden liberation of pressure by rupture, form an objection to this mode of working, and limits its application to situations where frequent tests are to be made of small vessels.

In such apparatus, a valve operating so as to automatically shut the flow from the accumulator as quickly as any rapid flow or fall of pressure occurs in the delivery pipe, is a useful adjunct.

Air vessels have been proposed and sometimes used in conjunction with pumps for this service, but the tendency to throw as projectiles, the fragments of the vessel after bursting it, renders such methods unsafe—it is, however, satisfactorily used in conjunction with strong surrounding guards or cages for the testing of glass bottles and fountains, made for charging, storing, and transporting aerated waters.

The inaccuracy of gauges for measuring high pressures is also a matter of interest in hydrostatic testing operations—none of those which depend on an expansible vessel or diaphragm, and a system of motion multiplying levers or sectors and pinions conveying motion to the index, are without the objection of friction of working parts, and under different conditions of lubrication are not only liable, but certain to vary.

The inaccuracy of pressure gauges of this type is so slight as to be unimportant in gauges for such pressures as those under which steam is ordinarily used, compared with those required for high pressures, such as are employed in testing for ultimate strength of vessels, and for the liquefaction of gases, which, by the by, is now practiced on a

commercial scale as an industry of considerable importance, when the inaccuracy is quite noticeable, and sometimes embarrassing in practice; the use of more than one gauge at the same time, arranged so as to be in view at once, serves as a measurably good precaution.

In the course of an experiment made before the members of this Institute, the two recording gauges were found to keep within twenty-five or thirty pounds of each other, and in testing them before and after the experiments with other gauges up to the highest pressure indicated in the experiment, they were found not to have set or changed in the elasticity of their springs.

The experimental test made in the presence of the members of the Institute is believed to have been adequate for the purpose for which it was intended, namely: to show that such structures as have been used, and are now in use, are not so unsafe as to call for any public alarm on the subject; but at the same time, that it was desirable to have more exact information than was accessible in publications on the subject, and the propriety of further inquiry by a series of experiments in such direction and an investigation, which investigation is now in progress.

In the experiment referred to, some other interesting matters appeared among others, those to which attention is directed in this paper.

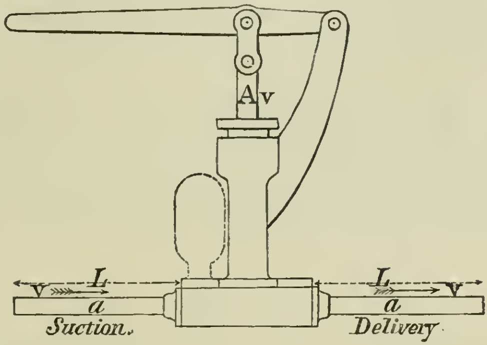
DISCUSSION.

Mr. Hugo Bilgram:—Considering that the transmission of strain in a fluid takes place with the velocity of sound in that fluid, and that therefore such transmission is practically instantaneous, the momentum of the water in the connecting pipe can hardly even temporarily increase the pressure in the boiler much beyond that due to the quantity of water forced into the same, and owing to the elasticity of the walls of the boiler the pressure is practically proportionate to the amount of water forced into it after it had been filled. The momentary pressure caused by the momentum of the water in the connecting pipe at the conclusion of any stroke of the pump can certainly not exceed the permanent pressure due to any of the following strokes.

Mr. J. W. Nystrom:—The momentum and force of impact of water moving in pipes are well known to hydraulic engineers, as can be seen in hydraulic machinery in which precautions for these forces

are properly made, and the science of which may be represented as follows:

The accompanying illustration represents a pump of plunger A, suction and delivery pipes, a, a.



A = cross area of plunger in square feet.
a = cross area of suction or delivery pipe.
v = velocity of plunger in feet per second.
V = velocity of water in the pipes per second.
L = length of pipe in feet.
W = weight of water in pipe in pounds.
g = 32·17 the acceleratrix of gravity.
F = force of impact in pounds.
T = time in seconds in which the impact acts.

Velocity of water in pipe $V = \frac{Av}{a}$. (1)

Momentum of motion $\frac{WV}{g} = FT$, momentum of time. (2)

Force of impact $F = \frac{WV}{gT}$. (3)

Insert formula (1) for V in formula (3), and we have

$$F = \frac{WAv}{gTa}. \quad (4)$$

Weight of water in pipe $W = 62{\cdot}4\, aL$. (5)

Insert formula (5) for W in formula (4), and we have

Force of impact $F = \frac{62{\cdot}4\ aLAv}{gTa} = \frac{62{\cdot}4\ LAv}{gT} = \frac{2\,LvA}{T}$. (6)

The acceleratrix g divided into the weight per cubic foot of water 62·4 pounds, is nearly 2.

The force F is also the additional force over the hydrostatic pressure required on the plunger A, for setting the water in motion in the pipe, in the time T, of one single stroke, omitting friction of the water in the pipe.

The force of impact as given by formula (6), is the whole force acting on the area a, where the motion of the water is stopped, and not the additional pressure per square inch caused by the force of impact.

$P =$ additional presssure per square inch caused by the force of impact.

$$P = \frac{2\,LvA}{Ta}. \qquad (7)$$

In this formula (7) the areas A and a can be expressed in square inches, but L must be expressed in feet. It is this formula which expresses the difficulty with momentum and concussion spoken of in Mr. Wiegand's paper. We find that this difficulty is proportionate to the length L, of the pipe, area A and velocity v of the pump plunger, and inversely as the time of impact and area a of the delivery pipe.

In the hydraulic boiler test made by Mr. Wiegand before the February meeting of the Institute, the delivery pipes were, as near as I can remember, about three-eighths of an inch inside diameter, and that of the plunger one inch, which makes the area of the plunger about seven times that of the pipe. The delivery pipe was entirely too small, and should have been at least one inch inside diameter, but $1\frac{1}{2}$ inch would be better.

The data in the experiment referred to, were about $L = 6$ feet, $v = 0{\cdot}5$, $A : a = 7$. $T = 0{\cdot}25$. Then the impact pressure would be as follows:

$$P = 2 \times 4 \times 6 \times 0{\cdot}5 \times 7 = 168 \text{ pounds per square inch.}$$

Now suppose the delivery pipe to be $1\frac{1}{2}$ inches diameter, and the other data remain as they were, then the impact pressure would be only

$$P = 2 \times 4 \times 6 \times 0{\cdot}5 \times 0{\cdot}444 = 10{\cdot}6 \text{ pounds per square inch.}$$

The elasticity of the vessel in which the water is forced, and the air in the water diminishes considerably the force of impact, but the

formulas, however, shows that the delivery pipe should be large in diameter for reducing the force of impact.

A steam pump for a marine railway at Portero, near San Francisco, California, had a suction pipe of about 400 feet long, and the force of impact of that column of water was so great that it finally burst the pipe. I then advised the proprietor, Mr. North, to put an air vessel on the suction side of the pump, as shown by the dotted line on the illustration, which was done, after which the plunger worked well.

The force of impact of a moving body has nothing to do with velocity of sound. Mr. Bilgram's statement that "the momentum of the water in the connecting pipe can hardly even temporarily increase the pressure in the boiler" is disproved by the operation of the hrdraulic ram.

VELOCITY OF APPROACH IN WEIR COMPUTATIONS.

TO FACILITATE THE COMPUTATION OF WEIR DISCHARGES, CORRECTED FOR VELOCITY OF APPROACH, BY A METHOD BASED ON THE FORMULÆ AND PRESCRIPTIONS OF THE LOWELL HYDRAULIC EXPERIMENTS.

By A. W. Hunking and Frank S. Hart, Lowell, Mass.

The following adaptation of the Francis formula for correcting weir discharges for velocity of approach was suggested by personal observation of the great amount of time consumed in the necessary weir computations pertaining to a general business of water measuring.

In an extended series of turbine tests like those published in Lowell Hydraulic Experiments, and in Journal Franklin Institute for April, 1875, involving the weir measurement of large quantities of water, with great depth on the weir, it is believed that the proposed method presents great advantages over the one commonly employed, both in brevity and accuracy.

The formula referred to, viz.:

$$Q = (L - 0{\cdot}1\, nH)\,[3{\cdot}33\,(H + h)^{\frac{3}{2}} - 3{\cdot}33\, h^{\frac{3}{2}}] \qquad (1)$$

may be found in Lowell Hydraulic Experiments, p. 252 (Ed. 1883).

The notation adopted is as follows:

Q represents the quantity of water discharged in cubic feet per second.
L " length of the weir, in feet.
H " height of water above crest of weir as observed at section S, in feet.
S " area of section of approach, in square feet.
v " velocity of approach, in feet per second.
h " $\frac{v^2}{2g}$ or head due velocity of approach, in feet.
n " number of end contractions.

To correct the discharge over the weir for the velocity of approach, by the above formula, the method commonly employed has been to make successive approximations to the value of Q; beginning with the formula [which is the same as equation (1), except that h is omitted, having been assumed as zero]

$$Q = 3{\cdot}33\,(L - 0{\cdot}1\;nH)\,H^{\frac{3}{2}} \tag{2}$$

and deducing from this value of Q, and the assumed section of approach of the current above the weir, an approximate value of h, to be used in equation (1), which, being solved, gives an increased value of Q, and this in turn may give a slightly increased value of h to use in a second solution of equation (1), and so continuing until the desired accuracy in the value of Q is obtained.

It is proposed to show in this paper that, to correct for the velocity of approach, a coefficient, K, depending upon the value of $\frac{h}{H}$ (where H is the observed depth on the weir, and h is the head due the *true* velocity), may be used, and that we may write

$$Q = 3{\cdot}33\,(L - 0{\cdot}1\;nH)\,H^{\frac{3}{2}}\,K, \tag{3}$$

in place of

$$Q = 3{\cdot}33\,(L - 0{\cdot}1\;nH)\,[(H + h)^{\frac{3}{2}} - h^{\frac{3}{2}}]. \tag{1}$$

When K can be taken from a table, or read directly from a diagram, it is obvious that the solution of equation (3) is far less laborious than the method first mentioned.

By reference to equations (1) and (3) it will be seen that $H^{\frac{3}{2}}\;K$ is substituted for $[(H + h)^{\frac{3}{2}} - h^{\frac{3}{2}}]$, making

$$K = \left(\frac{H + h}{H}\right)^{\frac{3}{2}} - \left(\frac{h}{H}\right)^{\frac{3}{2}} = \left[\left(1 + \frac{h}{H}\right)^{\frac{3}{2}} - \left(\frac{h}{H}\right)^{\frac{3}{2}}\right]. \tag{4}$$

By definition

$$h = \frac{v^2}{2g} = \frac{Q^2}{S^2 2g} = \frac{3{\cdot}33^2 (L - 0{\cdot}1\, nH)^2 (H^{\frac{3}{2}})^2 K^2}{S^2\, 2g} = c \left(\frac{1}{D}\right)^2 H^3 K^2$$

(c being $\dfrac{3{\cdot}33^2}{2g}$ and D being $\dfrac{S}{(L - 0{\cdot}1\, nH)}$): dividing this last equation by H gives

$$\frac{h}{H} = c \left(\frac{H}{D}\right)^2 K^2, \tag{5}$$

which being substituted in equation (4) gives as a value for K

$$K = (1 + c \left(\frac{H}{D}\right)^2 K^2)^{\frac{3}{2}} - (c \left(\frac{H}{D}\right)^2 K^2)^{\frac{3}{2}} \,. \tag{6}$$

By solving equation (6), values of K corresponding to $\left(\frac{H}{D}\right)$ for every half hundredth up to 0·36 (the limit of the formula) have been computed, furnishing the requisite data for constructing a suitable curve.

It is found that this curve may be obtained very nearly by the formula

$$K = 1 + 0{\cdot}2489 \left(\frac{H}{D}\right)^2,$$

in which (within the limits before mentioned) the error will not exceed $\frac{1}{200}$ of 1 per cent.

If an error of $\frac{1}{50}$ of 1 per cent. is admissible, the formula

$$K = 1 + \left(\frac{H}{2\,D}\right)^2$$

will be found better adapted to rapid computations with the aid of a table of squares.

In obtaining $\left(\frac{H}{D}\right)$ it should be borne in mind that $D = \dfrac{S}{(L - 0{\cdot}1\, nH)}$, as above.

In case there are no end contractions, n becomes zero, and $D = \dfrac{S}{L}$ = depth of channel of approach.

Lowell, Mass., April 15th, 1884.

TABLE

SHOWING THE VALUE OF K COMPUTED BY THE FORMULA

$$K = (1 + c\left(\frac{H}{D}\right)^2 K^2)^{\frac{3}{2}} - (c\left(\frac{H}{D}\right)^2 K^2)^{\frac{3}{2}}$$

$\left(\frac{H}{D}\right)$	K	$\left(\frac{H}{D}\right)$	K	$\left(\frac{H}{D}\right)$	K
0·000	1·000000	0·125	1·003933	0·245	1·014931
·005	1·000006	·130	1·004251	·250	1·015543
·010	1·000026	·135	1·004581	·255	1·016167
·015	1·000058	·140	1·004923	·260	1·016805
·020	1·000103	·145	1·005278	·265	1·017455
·025	1·000161	·150	1·005644	·270	1·018117
·030	1·000231	·155	1·006023	·275	1·018792
·035	1·000314	·160	1·006414	·280	1·019480
·040	1·000409	·165	1·006817	·285	1·020180
·045	1·000518	·170	1·007232	·290	1·020893
·050	1·000638	·175	1·007659	·295	1·021620
·055	1·000772	·180	1·008099	·300	1·022359
·060	1·000917	·185	1·008551	·305	1·023110
·065	1·001075	·190	1·009015	·310	1·023875
·070	1·001246	·195	1·009491	·315	1·024653
·075	1·001429	·200	1·009980	·320	1·025444
·080	1·001624	·205	1·010480	·325	1·026248
·085	1·001832	·210	1·010994	·330	1·027065
·090	1·002051	·215	1·011519	·335	1·027895
·095	1·002284	·220	1·012057	·340	1·028739
·100	1·002528	·225	1·012607	·345	1·029596
·105	1·002785	·230	1·013169	·350	1·030467
·110	1·003053	·235	1·013744	·355	1·031350
·115	1·003335	·240	1·014331	·360	1·032248
·120	1·003628				

EXAMPLE.

DATA.

$H = 1{\cdot}954$ ft. $L = 11{\cdot}22$ ft. $n = 0$
$S = L\,D = L\,(1{\cdot}954 + 5{\cdot}640) = 11{\cdot}22 \times 7{\cdot}594$ } log. L 1·0499929
" D 0·8804706

" S 1·9304635

PRESENT METHOD.

0·9588311	log. $3{\cdot}33\,H^{\frac{3}{2}}$
1·0499929	" $L = 11{\cdot}22$
2·0088240	" $Q_1 : Q_1 = 102{\cdot}0526$
1·9304635	" S as above.
0·0783605	" $V_1 : V_1 = \frac{Q_1}{S}$
0·1567210	" V_1^2
1·8083703	" $2g$
8·3483507	" $h_1 = \frac{V_1^2}{2g} : h_1 = {\cdot}02230$
5·0450521	
7·5225260	$h_1^{\frac{3}{2}} : h_1^{\frac{3}{2}} =$ ·00333
	$H =$ 1·95400
	$h_1 =$ 0·02230
0·2958529	log. $H + h_1 = 1{\cdot}97630$
0·8875587	
0·4437793	" $(H + h_1)^{\frac{3}{2}} =$ 2·77830
0·4432583	" $(H+h_1)^{\frac{3}{2}} - h_1^{\frac{3}{2}} = 2{\cdot}77497$
1·0499929	log. L
0·5224442	" 3·33
2·0156954	" $Q_2 : Q_2 = 103{\cdot}6801$
1·9304635	" S
0·0852319	" $V_2 \quad \frac{Q_2}{S}$
0·1704638	" V_2^2
1·8083703	" $2g$
8·3620935	" $h_2 : h_2 = {\cdot}02302$
5·0862805	
7·5431402	log. $h_2^{\frac{3}{2}} : h_2^{\frac{3}{2}} = {\cdot}00349$
	$H =$ 1·95400
	$h_2 =$ 0·02302
0·2960111	log. $H + h_2 = 1{\cdot}97702$
0·8880333	
0·4440166	" $(H + h_2)^{\frac{3}{2}}$ 2·77982
0·4434711	" $(H+h_2)^{\frac{3}{2}} - h_2^{\frac{3}{2}} = 2{\cdot}77633$
1·0499929	log. L
0·5224442	" 3·33
2·0159082	" $Q_3 : Q_3 = 103{\cdot}7309$
1·9304635	" S
0·0854447	" V_3
0·1708894	" V_3^2
1·8083703	" $2g$
8·3625191	" $h_3 = \frac{V_3^2}{2g} : h_3 = {\cdot}02304$
5·0875573	
7·5437786	" $h_3^{\frac{3}{2}} : h_3^{\frac{3}{2}} = {\cdot}00350$
	$H =$ 1·95400
	$h_3 =$ 0·02304
	$(H+h_3) = 1{\cdot}97704$
0·2960155	log. $H + h_3 = 1{\cdot}97704$
0·8880465	
0·4440232	" $(H + h_3)^{\frac{3}{2}} = 2{\cdot}77986$
0·4434758	$(H+h_3)^{\frac{3}{2}} - h_3^{\frac{3}{2}} = 2{\cdot}77636$
1·0499929	log. L
0·5224442	" 3·33
2·0159129	" $Q_4 : Q_4 = 103{\cdot}7320$
1·9304635	" S
0·0854494	" V_4
0·1708988	" V_4^2
1·8083703	" $2g$
8·3625285	" $h_4 : h_4 = 0{\cdot}2304$

EXAMPLE Continued.

Since h_4 is the same as h_3, it is evident that the value of Q_5 will be the same as Q_4. Therefore

$$Q = 103{\cdot}7320 \ c. f. p. s.$$

PROPOSED METHOD.

0·2909246 log. H
0·8804706 " D

9·4104540 " $\left(\frac{H}{D}\right)$ whence $\left(\frac{H}{D}\right)$
$= 0{\cdot}2573$

Corresponding value of K by interpolation, 1·01646.

0·0070903 log. K
0·9588311 " 3·33 $H^{\frac{3}{2}}$
1·0499929 " L

2·0159143 " Q, whence

$$Q = 103{\cdot}7324$$

Bernauf's Telescope.—Capt. Bernauf has invented a very simple and very convenient instrument, which will often be useful for sailors. When the ocean horizon is clouded, it is difficult to measure the altitude of a star. The difficulty is obviated by a circle of iron, to which a small telescope is attached. Along the circle, in a groove, is placed a tube containing mercury. The metal occupies the lower part of the tube and oscillates according to the positions which the circle takes in the had of the observer, as it would do in the two branches of a U tube. In every position, however, the straight line, along the surfaces of the mercury in the two branches, is horizontal. The observer takes the circle in his hand, directs the telescope towards the portion of the sky which he desires to explore, and when he holds the star in the field of vision, he immediately fixes the mercury in its place by touching a button which controls an ingenious mechanism. He has then only to read, on a graduated circle, the angle formed between the direction of the telescope and that of the horizontal line.—*Les Mondes*, April 5, 1884. C.

Microscopic Organisms on the Surface of Coins.—P. F. Reinsch, having had occasion to make microscopic observations of some small silver coins, found numerous bacteria and some very characteristic algæ in the thin incrustations. Extending his observations to the copper, bronze, and gold coins, of various nations and of different values, he found that similar organisms seemed to be always present, after the coins had been in use for a year or more. They can be readily seen by scraping the surface with the point of a knife, putting the portion removed in a drop of distilled water and spreading it, with a clean knife, under a microscope which magnifies from 250 to 300 times. This discovery seems important, not only in biology, but also in the practical relations of hygiene, especially in consideration of the germ theory of disease. A portion of the erosion of the surface of coins, may perhaps be due to these organisms.—*Dingler's Journal*, March 26, 1884. C.

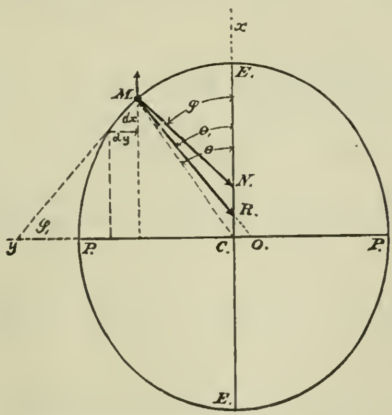
THE EARTH'S ELLIPTICITY.

By L. D'AURIA.

The present paper is intended as a continuation and an amendment of the one already published on the Ellipticity of Planets in the JOURNAL OF THE FRANKLIN INSTITUTE for May, 1884, in which, from a well-established principle of Mechanics, the following equation is deduced for the relative equilibrium of an oblate fluid ellipsoid of revolution rotating around its minor axis:

$$e^2\,(2 - e^2) = \text{tang.}^2\,\theta.\ \text{cotang.}^2\,\theta_1\ \frac{w^2\rho}{g}\left(2 - \frac{w^2\rho}{g}\right) + \frac{\sin.^2\theta - \sin.^2\theta_1}{\cos.^2\theta\,\sin.^2\theta}; \qquad (1)$$

In this e, represents the eccentricity of the generating ellipse; g, the attraction at any point M of the free surface; ρ, $= MO$, the distance



on the direction of g, between the point M and the axis of rotation; w, angular velocity of rotation; θ, the angle made by the radius MC with the equatorial plane; and θ_1, the angle made by the direction of ρ, with the same plane.

The angle θ_1 is assumed smaller than the latitude φ of the same point M; this condition being absolutely necessary in order that the resultant action of attraction and centrifugal force may be normal to the free surface at M; but no definite value has been assigned to

it. If the component attractions X and Y of the point M, respectively parallel to the axes x and y, are known, then

$$\text{cotang.}^2\,\theta_1 = \frac{X^2}{Y^2};\ \sin.^2\,\theta_1 = \frac{Y^2}{X^2 + Y^2};$$

and expressing tang. θ, sin. θ, cos. θ, in terms of the co-ordinates x and y; and putting

$$\rho = \frac{x}{\cos.\,\theta_1} = \frac{x}{X}\sqrt{X^2 + Y^2},$$

we can write equation (1) as follows:

$$e^2\,(2 - e^2) = \frac{y^2 X^2}{x^2 Y^2}\,\frac{x\,w^2}{X g}\sqrt{X^2 + Y^2}\,\Big(2 - \frac{x\,w^2}{X g}\sqrt{X^2 + Y^2}\Big) + 1 - \frac{y^2 X^2}{x^2 Y^2}. \qquad (2)$$

Observing that in comparison with the number 2 in brackets, the quantities e^2 and $\frac{x\,w^2}{X g}\sqrt{X^2 + Y^2}$ may be neglected, we can put approximately

$$2\,e^2 = 2\,\frac{y^2 X^2}{x^2 Y^2}\,\frac{x\,w^2}{X g}\sqrt{X^2 + Y^2} + 1 - \frac{y^2 X^2}{x^2 Y^2}. \qquad (3)$$

Now for a homogeneous oblate ellipsoid we have

$$X = \frac{2\pi\,D\,b^2}{a^2 - b^2}\left\{\frac{a^2}{b\,\sqrt{a^2 - b^2}}\,\tan.^{-1}\,\frac{\sqrt{a^2 - b^2}}{b} - 1\right\}x,$$

$$Y = \frac{4\pi\,D\,a^2}{a^2 - b^2}\left\{1 - \frac{b}{\sqrt{a^2 - b^2}}\,\tan.^{-1}\,\frac{\sqrt{a^2 - b^2}}{b}\right\}y.$$

Expressing these values in terms of the eccentricity e, neglecting e^4 and higher powers of e, we have

$$X = \tfrac{4}{3}\pi D\,(1 - \tfrac{1}{5}e^2)\,x;\ \ Y = \tfrac{4}{3}\pi D\,(1 + \tfrac{2}{5}e^2)\,y.$$

Substituting in equation (3) we obtain

$$2\,e^2 = 2\,\frac{(1 - \frac{1}{5}e^2)^2}{(1 + \frac{2}{5}e^2)^2}\,\frac{w^2}{(1 - \frac{1}{5}e^2)\,g}\sqrt{x^2\,(1 - \tfrac{1}{5}e^2)^2 + y^2\,(1 + \tfrac{2}{5}e^2)^2} + \tfrac{6}{5}e^2;$$

and putting $y = o$, x will $= a$ and $g = g_1 =$ attraction at the equator, and therefore

$$e^2 = \frac{(1 - \frac{1}{5}e^2)^2}{(1 + \frac{2}{5}e^2)^2} \frac{w^2 a}{g_1} + \frac{3}{5}e^2;$$

or approximately

$$e^2 = \tfrac{5}{2} \frac{w^2 a}{g_1}; \ \varepsilon = \tfrac{5}{4} \frac{w^2 a}{g_1}.$$

In the case of the earth we have

$$\frac{w^2 a}{g_1} = \frac{1}{289};$$

hence

$$\varepsilon = \frac{1}{230},$$

which is exactly Newton's value given in Principia, likewise determined on the hypothesis of the earth being homogeneous.

It can be seen from the foregoing that equation (1) is perfectly sound and no element or necessary condition has been neglected in deriving it; but in assigning values to the quantities

$$\text{tang.}^2\,\theta,\ \text{cotang.}^2\,\theta_1 \text{ and } \frac{\sin.^2\,\theta - \sin.^2\,\theta_1}{\cos.^2\,\theta \ \ \cos.^2\,\theta},$$

when $\theta = \theta_1 = o$, we were deceived by their form, and thought that in this case their respective values would be 1 and o; hence the discrepancy found for the earth's ellipticity.

Having corrected this error, and made sure of the soundness of the fundamental equations, we shall touch upon a point of the utmost importance in physical astronomy.

The formulæ given above for the component attractions at a surface point of a homogeneous oblate ellipsoid cannot be applied to planets on account of the heterogeneity of the masses. But knowing from observations on our planet, that, irrespective of centrifugal force, the attraction at the surface increases in a regular manner from the equator to the poles, though not in the same proportion as if the earth were homogeneous, we can, for the degree of approximation required in this investigation, represent the component attractions at a surface point of a planet composed of strata of different densities, by

$$X = xf(e); \ \ Y = yf_1(e);$$

where x and y are the co-ordinates of the attracted point, and e, the eccentricity of the generating ellipse of the surface. However, for

more generality, it may be supposed that the functions f and f_1 contain also the eccentricities of internal strata, as well as the densities of the latter, if we please, without interfering with the result at which we aim.

Denoting by g_1 and g_2 the attractions at the equator and at the poles respectively, and substituting in equation (3) the last values of X and Y, we find for a point at the equator

$$2e^2 = 2\,\frac{w^2 a}{g_1}\,\frac{f(e)^2}{f_1(e)^2} + 1 - \frac{f(e)^2}{f_1(e)^2}; \tag{4}$$

and for a point at one of the poles

$$2e^2 = 2\,\frac{w^2 b}{g_2}\,\frac{f(e)}{f_1(e)} + 1 - \frac{f(e)^2}{f_1(e)^2}.$$

Hence,

$$\frac{g_1}{g_2} = \frac{a}{b}\,\frac{f(e)}{f_1(e)};$$

and putting $b = a(1-\varepsilon)$,

$$\frac{g_1}{g_2} = \frac{f(e)}{(1-\varepsilon)f_1(e)}.$$

On the other hand, by putting in equation (4)

$$e^2 = 2\varepsilon;\ \frac{w^2 a}{g_1} = j;$$

it becomes

$$\frac{f(e)}{f_1(e)} = \frac{(1-4\varepsilon)^{\frac{1}{2}}}{(1-2j)^{\frac{1}{2}}},$$

hence

$$\frac{g_1}{g_2} = \frac{(1-4\varepsilon)^{\frac{1}{2}}}{(1-\varepsilon)(1-2j)^{\frac{1}{2}}}.$$

Letting $\frac{g_2}{g_1} = k$, and solving for ε, we have finally

$$\varepsilon = \tfrac{1}{2}\,\frac{k^2 + 2j - 1}{2k^2 + 2j - 1}. \tag{5}$$

This is a very valuable equation since it gives the ellipticity of a planet composed of strata of different densities, when the ratios k and j are determined by observation.

Now we know that in the case of the earth,

approximately, $k = \frac{600}{599}$, and $j = \frac{1}{289}$;

substituting in equation (5) we find

$$\varepsilon = \frac{1}{197}$$

Since we know that at present the actual value of ε does not vary greatly from $1 \div 300$, and that the values of k and j cannot be distrusted, the discrepancy here implied must have a deep meaning, if we are correct in our mathematics. In order to investigate this meaning, let us see what change the ratio k should undergo to make ε agree with the earth's known ellipticity. It will be observed that by simply making $k = 1$ instead of $k = 600 \div 599 = 1{\cdot}00167$, we obtain

$$\varepsilon = \frac{1}{291};$$

hence a very slight diminution of k is sufficient to produce the desired effect. Let us suppose that the ratio k is not a constant quantity, but is really subject to a very slow secular change which constantly increases its value. Assuming then that the earth's mass became compact enough to retain its form at a time when the ratio k was very near or equal to unity; no discrepancy existed at that time between the theoretical and the actual value of ε; but at a later period the increase of k was not followed by a corresponding increase of ε, because the mass of the earth no longer yielded to the new conditions of the forces acting upon its particles. By aid of the assumptions above made the discrepancy above becomes easily accountable, and can be advanced as a forcible argument in favor of the present rigidity of the earth's interior.

Magnetism in Madagascar.—Lieut. Hallez, when stationed at Madagascar, sent a communication to the International Society of Electricians, in regard to the daily storms which are experienced upon the island. On approaching the island, the soil of which is very furruginous, the compass undergoes considerable and very abnormal deviations. This fact has induced many persons to consider Madagascar as an enormous magnet. Lieut. Hallez proposes to undertake a series of systematic observations and to communicate the results to the Society.—*Les Mondes*, March 29, 1884. C.

SUGGESTIONS FOR IMPROVEMENT IN THE MANUFACTURE OF GLASS

AND OF NEW METHODS FOR THE CONSTRUCTION OF LARGE TELESCOPIC LENSES.

By George W. Holley.

[Read in Section A (Physics), at the meeting of the American Association for the Advancement of Science, held at Cincinnati, Ohio, August, 1881.]

It is many years since Sir David Brewster very confidently expressed the opinion that before the end of the present century the world would possess a refracting telescope with an object glass two feet in diameter, and a reflecting telescope the mirror of which would be twenty feet in diameter. And no individual of his time, by reason of his extensive scientific attainments and researches, and his exalted character as a man, was more entitled to speak authoritatively on this subject. Having read the glowing records of the sky with the patient zeal of the scholar and the devout ardor of the christian, he extended the boundaries of our knowledge by his discoveries, and enriched our literature by his writings. Undoubtedly it was his knowledge of the powers and capabilities of optical instruments that led to the expression of the opinion just quoted.

In reference to the refractor, Sir David's anticipations have been more than realized, while in reflectors only two improvements have been made: one in the manner of mounting and manipulating the speculum, the other in the process for silvering its face. Great improvements have also been made in the mechanical processes for handling, shaping and finishing all kinds of material substances, and of melting in large masses all fusible matter. By reason of these improvements it has become possible to construct a metallic speculum of the size mentioned by Sir David. But there are so many reasons why refracting instruments should be preferred, that it is not probable that any important efforts will be made in this direction, until exhaustive experiments have demonstrated the impossibility of vast improvement in refractors.

The present inquiry, therefore, will be directed, as regards lenses, to the consideration of methods by which telescopic object glasses may be greatly increased in size and improved in efficacy.

The first requisite to success is the ability to manufacture pure,

homogeneous glass. The next is to make this into lenses that shall properly concentrate, transmit and sift, so to speak, the solar rays, the most beautiful and exacting of all the imponderables. In 1776, M. Brisson in a report to the French Academy, on the results of experiments made with the Trudaine lens, so-called, says: "We ought to consider it impossible to make a perfect glass lens of large size." Sir David Brewster in his treatise on "New Philosophical Instruments," referring to the astronomical telescope, says: "The imperfection of this instrument arises from two causes; from the partial correction of color which is a consequence of an inequality in the colored spaces of the spectra produced by crown and flint glass, and from the difficulty of procuring flint glass free from veins or specks."

While the votaries of science in view of their triumphs during the last fifty years will be reluctant to admit the word "impossible" into their vocabulary of progress, still it must be admitted that M. Brisson's incredulity was quite justifiable since we stand to-day, more than a century after his prophetic utterance, in almost hopeless contemplation of the problem, how we are to obtain pure, homogeneous glass in the desired masses if we are to depend upon the old methods of manufacture. A slight increase in the diameter and thickness of the unground lens greatly increases the difficulty of securing homogeneity in the mass. It is said that the stewards of the magnificent bequest of Mr. James Lick, with ample funds in hand to raise, in the pure atmosphere of some one of the Rocky Mountain summits, an instrument far superior to any now in existence, are standing with folded hands unwilling to go forward because they have no assurance that even moderate success will reward their expenditure and satisfy the wish of their generous patron. The largest lens that the world renowned opticians, the Messrs. Clark and Sons, of Cambridge, were willing to undertake for Prof. Struve in behalf of the Emperor of Russia, to be placed in the famous observatory at Pulkova, is to be only 31 inches in diameter, so the world waits for its great telescope. With genius, gold and good-will to aid the grand scheme it would seem that success should be assured. Are we to be fettered and foiled by the old methods and practices whose maximum of utility seems to have been long since exhausted? Let us consider some facts that make for the negative of this question. And first the process of glass making demands attention. It can hardly be said to have been improved since the time of Dolland, Frauenhofer and Guinaud, when England, Germany

and France were honorable and earnest competitors in the good work. But the *quality* of the glass has been improved because it is made of better material—the silicious sand of Massachusetts, the purest bed of which, known to the world, was discovered some years since in Berkshire county in that State. This is now sent to the manufacturers of the finest glass in England and on the Continent, and although they cannot yet make it pure in masses large enough to satisfy the desires of the most advanced opticians, still it is true that thin plates of glass of great size and purity can be made. It is only necessary to look into the magnificent mirrors which adorn the dwellings of some of our wealthy citizens, to be convinced of this fact. It is also true that *bars* of glass of great purity, from two to four inches square, and from ten to twenty inches long can be made.

Mr. Charles Tomlinson in his "Cyclopedia of Useful Arts and Manufactures" mentions the fact that "agitation of glass, while in a liquid state, improves its quality," and it is believed that this discovery was made by the Dolland's and is the secret of their great success in glass-making. It is also believed that the best living manufacturers of glass for optical purposes, the Messrs. Glance, of Birmingham, England, are indebted to the same secret for their eminient success in the art. But the present arrangement of melting pots and ovens is such as to render thorough agitation almost impossible and also restricts, within narrow limits, the size of the masses of glass that can be produced.

A remedy for these hitherto insurmountable difficulties seems to be offered by the use of a most important modern invention, the rotating gas furnace which produces the highest available temperature—about 4,600° F.—, and will supply the largest masses of metal and at the same time secure any degree of agitation that may be desired. As a general rule it is certain that the more thoroughly liquid a metal can be made the more likely it is to be pure in quality and homogeneous in structure. The benefit resulting from the improved process for silvering the surface of glass reflectors has recently been demonstrated by the superior speculum constructed for Mr. A. Ainslee Common which is 37½ inches in diameter, is mounted at Ealing and has proved a decided success. The first speculum made for him, after it was just ready for mounting was lost by bursting "into a thousand pieces," a calamity that can only happen to those that are made of solid glass. By the methods of construction hereinafter proposed such a misfortune would be impossible.

The silvering of the surface of glass, to improve its refractive power,

naturally suggests the introduction of the metal, in some form, in the manufacture of the glass, as has long been done with lead and other metals. Doubtless experiment would demonstrate that by using the metallic silver properly comminuted, or some of its compounds, in making the glass the refractive power of the latter and its optical value would be very greatly increased.*

Having secured glass of the desired purity the next step is to make the lens. From the ability to procure much larger masses of pure glass, by the use of the new style of furnace, than have hitherto been attainable, results in the possibility of making much larger solid lenses than have ever yet been attempted. Whether the maximum of success can be obtained in this direction experiment only can determine. But it is beleived that maximum can best be obtained by adopting an entirely new method or methods of lens construction. Before describing them a few preliminary observations will be useful. Many persons who have bad occasion to use opera glasses and spectacles have noticed that a cleavage or crack in the lenses does not injure their power to produce correct images provided the edges of the crack are not crap'd or ragged. The writer used for some years a telescope, the object glass of which had a crack entirely accross it. But it was not perceptible to the eye when directed to a distant object nor did it impair the image or produce unusual diffraction of the solar rays. We may also note the fact that the firm adhesion of different parallel surfaces of glass, after they have been properly prepared, is secured by the use of transparent cements which do not impair their refractive power. It may be further noted that the sand blast, recently utilized, is used by the Messrs. Clark in shaping unground lenses, which process they also find to be greatly facilitated by the use of the chilled cast iron globules introduced by Mr. B. C. Tilghman, of Philadelphia.

Such being the facts, it is proposed to make the pure silver-bearing glass into bars, two or three inches square and ten to twenty inches long, or such size as experiment shall prove to be best; and, after properly testing every bar, to select for use only those that prove to be absolutely pure and homogeneous. But for this experiment, bars of the very pure glass, made by the Messrs. Glance, could be used, and if

* It may be that M. M. Feil and Son of Paris, who have experimented extensively and skillfully in glass-making have made experiments in this direction; but if so the writer has met no account of them.

the experiment proved a success—if a perfect lens of a given power should be constructed—then further effort to secure solid lenses of the same or greater power would be unnecessary.

President Barnard, of Columbia College, having acted as one of the United States Commissioners to report upon the Paris Exposition in 1867, and the mechanical department and instruments of precision having been assigned to him, states in his report that M. Steinheil, of Munich, exhibited "hollow prisms" . . . that "were formed of plates of plane glass" and "united without cement, being made water-tight by the perfection and polish of their surfaces," a most important fact bearing upon this inquiry, since this perfect finish, in addition to the use of cement, would insure an adhesion of the surfaces of plates or bars which it would be difficult to overcome.

Having secured a sufficient number of bars of the highest attainable purity and finish, let the cement be applied, and then lay or pile them together like cordwood, until a block of any required size is obtained. Let these be bound firmly together with steel hoops, or otherwise, and afterward shaped and finished as may be desired. It is not supposed that the slight amount of polarized light which would be produced around the circumference by the strongest pressure would effect the functions of the lens. Large and thin plates of the same kind of glass could be prepared and cemented together in a similar manner, the largest plate being placed in the middle of the pile, with those on the two sides of it diminishing somewhat in diameter, until the neces-sary thickness should be obtained, after which they could be shaped and finished. When finished their surfaces would present a series of concentric rings on each side of the middle plate. Lenses made after either of these plans would not be in danger of destruction by such an accident as occurred to Mr. Common's large speculum, before noticed, since they could not burst from unequal expansion or contraction.

It will be observed that the two methods of construction here proposed are suggested by that most beautiful piece of mechanism the human eye. In proof of this, it is only necessary to note some elementary facts concerning the structure of the eye. Its globe, or ball, is enclosed in a wall composed of three membranes—the sclerotic, choroid and retina. Its outer lens—the cornea—consists of several concentric layers of transparent, homogeneous matter. The choroid is a thin membrane which adheres loosely to the sclerotic, except at a

single point—the oculus fundus. It is void of fibres, but is traversed by numerous minute blood vessels, which intersect in all directions, forming a complete network over all its posterior surface. The retina is a thin, very delicate membrane, which lines, without adhering to, the choroid. It is translucent, serving as it were the purpose of a silver coating for the reception of the luminous rays. Behind this is the black pigment, so called, which serves as a cushion to receive the rays of light, and to absorb, within certain limits, the superfluous rays. This pigment also makes the retina a mirror,* a fact not before mentioned, so far as the writer knows, perhaps because it is so obvious that other writers have not thought it necessary to mention it.

Another interior membrane is the hyaloid, which encloses the vitreous humor. It is not a continuous, simple substance, but is partitioned off into numerous cells, which contain the fluid. Even the crystalline lens—the most solid portion of the eye—is composed of three parts, a bulb-like capsule enclosing a central lens, which is a combination of concentric plates.

Thus we learn that, in its whole structure, the eye presents almost innumerable surfaces to the luminous rays. This is a special and most prominent feature in its construction. May it not be possible that we have hitherto failed to recognize its significance, that the chief characteristic of a method of construction which an optician would condemn is the very one that the Divine Architect designed should both prevent spherical aberration, and secure, as far as possible, the achromatization of the solar rays? The methods of constructing lenses herein proposed, involve precisely the same structure in a modified form. And while it is not always safe to reason from the less to the greater, still experiment may prove it to be true that these methods of combining a number of surfaces which may be presented to the solar rays at different angles, will cause them to be so blended and mingled that both aberration and prismatic discoloration may be practically neutralized. Whether the principles involved in the method of construction adopted by the Creator were correct or otherwise, they seem to have been controlling, and we cannot go far wrong in the endeavor to copy his work.

Summarily stated the ends herein proposed to be attained are:

1. To improve the quality of glass by introducing silver into its composition, and to increase the quantity that can be produced at a

* As is proved by the Opthalmoscope.

single melting by using the rotating gas furnace which will secure any required degree of agitation of the liquid mass.

2. While the attempt to make large solid lenses may be successful, still to make the effort to secure better results by adopting the new methods of construction herein described.

That the difficulties connected with the undertaking are formidable cannot be denied, but that they are insurmountable, who will venture to assert, after recalling to mind the physical and mechanical triumphs of the last half century? If success shall crown earnest and well-directed effort, it will be worth to the world all possible cost.

SURVEYS FOR THE FUTURE WATER SUPPLY OF PHILADELPHIA.*

By RUDOLPH HERING. C. E., *Assistant in Charge.*

PHILADELPHIA WATER DEPARTMENT,

January 27, 1884.

COL. WILLIAM LUDLOW, *Chief Engineer.*

SIR:—I have the honor to present to you herewith the following report of the progress of the Surveys for the Future Water Supply of the City of Philadelphia, during the past year.

In accordance with your instructions, the first object was to collect the information already in existence, and also all suggestions covering the question of increasing and improving the present supply, and to make a general examination into the entire subject.

The results of this preliminary inquiry were as follows, the information being recorded as nearly as possible, in chronological order:†

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* Annual Report of the Chief of the Water Department of the City of Philadelphia, 1883.

† List of publications relationg to the subject of the Future Water Supply of the City of Philadelphia:

1. Annual Reports of the Water Department, from 1856 to 1882.

2. Reports of a special committee of the Commissioners of Fairmount Park upon the Preservation of the Purity of the Water Supply, October, 1867.

3. Proposition of certain manufacturers of Manayunk for supplying the City of Philadelphia with pure water, contained in a memorial to the State Legislature, February, 1868.

As early as 1856, efforts were made to secure a better water supply for the city. The Schuylkill valley was being rapidly built up above the city, and the quality of the water, owing to the additional refuse cast into it, was becoming correspondingly less pure.

In 1858, the Chief Engineer of the water Department, Mr. H. M. P. Birkinbine, urged that other sources than the Schuylkill be investigated, and made a preliminary report on the subject.

He considered the Wissahickon, the Delaware and the Lehigh at Easton, also the Schuylkill above Reading; rejected them all as unsuitable, and recommended an examination into the small watersheds about the city.

In 1863, I. S. Cassin, Chief Engineer of the Water Department, in his annual report, likewise urged the necessity for securing a better supply.

In 1864, Mr. Birkinbine, again Chief Engineer, was granted an

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4. On the Water Supply of Philadelphia. Pamphlet, by James Haworth, 1871.

5. Memorial to City Councils on supplying the City of Philadelphia with water from the Schuylkill and Wissahickon, by James Haworth, 1875.

6. On the Water Supply of Philadelphia. Pamphlet, by J. W. Nystrom, 1875.

7. Report on the Present and Future Water Supply for the City of Philadelphia, made to Councils by a Commission of Engineers, 1875.

8. Rainfall on the Basin of the Schuylkill River. Paper, by H. M. P. Birkinbine, JOURNAL FRANKLIN INSTITUTE, March and May, 1876.

9. Future Water Supply of the City of Philadelphia. Two papers, by H. M. P. Birkinbine, JOURNAL FRANKLIN INSTITUTE, May and July, 1878.

10. The Water Supply of Philadelphia. Paper by Charles G. Darrach, Proceedings of the Engineers' Club, Philadelphia, May, 1879.

11. The Future Water Supply of the City of Philadelphia. Paper, by James F. Smith, JOURNAL FRANKLIN INSTITUTE, October, 1879.

12. The Future Water Supply of Philadelphia. Paper, by H. M. P. Birkinbine, JOURNAL FRANKLIN INSTITUTE, November, 1879.

13. The Philadelphia Water Supply. Report of a Commission to James Haworth, 1880.

14. Report on Drainage from the Falls of Schuylkill, November, 1882, and on the Pollution of the Schuylkill River, January, 1883, by Russell Thayer, Superintendent of Fairmount Park.

15. Report on the Pollution of the Schuylkill River, etc., to the Commissioners of Fairmount Park, by Dr. Charles M. Cresson, January, 1883.

16. Potability of the Schuylkill Water Supply. Report to a Board of Experts, by Professor A. R. Leeds, January, 1883.

17. Reports on the Philadelphia Water Supply, made to Councils, by a Board of Experts, October, 1882, and April, 1883.

appropriation to make surveys for a supply of water to be brought from beyond the limits of the city, which resulted in a reconnoissance of all the creeks and streams within a radius of 40 Miles.

The following year he submitted a report. He had examined the Chester, Ridley, Crum, Darby, Cobb's, Mill, Gulf and East Valley creeks on the west side of the Schuylkill; and the Wissahickon and Plymouth creeks, the Sawmill Run, Stony and Perkiomen creeks on the east side.

The report gives the quantity of water available from each, the location of storage reservoirs, etc., and concludes with recommending a gravity supply from the Perkiomen, with a delivery into the city at an elevation of 175 feet above datum, from a storage reservoir to be located above Schwenksville, requiring a dam across the valley 65 feet high, which would impound the water from an area of 220 square miles.

The scheme is advocated with some force and supported by a large number of data, as far as they were available at the time. If sufficient storage capacity were provided, it was claimed that 240 millions of gallons daily could be furnished to the city from this source.

In 1866, the Fairmount Park Commission was created to secure such lands along the Schuylkill and Wissahickon as might be necessary to prevent the pollution of both streams, and to convert them into a public park. By this means it was expected to maintain the purity of the water supply.

On account of the objections that had been raised against the Perkiomen project by the Fairmount Park Commissioners and others, Mr. Birkinbine in his report to Councils for the year 1866, again discusses the scheme and endeavors to answer the disputed points by giving additional facts.

From a number of comparisons with similar works he confidently estimates the daily average supply from the proposed reservoirs at Zieglersville to be at least 150 million gallons. He also states that the water in this basin cannot become stagnant on account of its size and depth, but that it will rather be improved by allowing the suspended matter to settle. He finally argues that the valley offers no inducements for factories, and therefore no elements for a pollution of the water.

In 1867, a Special Committee of the Fairmount Park Commission, consisting of Fred. Graff, John C. Cresson, George G. Meade, Strick-

land Kneass and William Sellers, reported on the preservation of the purity of the city's water supply, with the conclusion that the Schuylkill river can be relied on for many years if proper means be taken early to guard it from pollution, especially by building an intercepting sewer from Manayunk to below the Fairmount dam, and if large retaining compensating reservoirs are built in the Upper Schuylkill to supply additional water during droughts.

By this latter means it was computed that the average flow of the river would give sufficient water power to raise into distributing reservoirs at Philadelphia over 116 million gallons per day through the driest period of the year.

In the following year, 1868, a bill was presented to the State Legislature, providing for the maintenance of the purity of the Schuylkill river between Norristown and Fairmount. While pending, a memorial was sent to the same body by a number of manufactures in Manayunk, protesting against the passage of this bill, and recommending a plan which, it was thought, would accomplish the same object, namely, to supply the City of Philadelphia with water from Flat Rock Dam, by means of a conduit extending from this dam to the pumps at Fairmount.

From other quarters it was suggested, instead, to build an open canal along one or both banks of the Schuylkill, by forming an embankment in it, and thus carry the refuse water to below the dam, and use the river water for the city's supply.

Owing to the opposition, the above bill did not pass; but neither of the projects was carried out in its stead.

For several years thereafter, no action was taken in the matter of improving the quality of the supply. The increased quantity of water required was supplied by increased steam-power.

This action called forth a series of pamphlets from a citizen, James Haworth, who was eager to show that the city could be furnished with the required quantity, at a much smaller cost, by water power only, and suggested the construction of numerous impounding dams to store the water from heavy rains.

In 1874, Dr. William H. McFadden, then Chief Engineer of the Water Department, discussed the question of the future water supply. Eliminating from consideration the plan of bringing water from the Delaware Water Gap by gravity, on account of its cost, he also, for the same reason, regards it as folly to bring it from New Hope, a point

that had been suggested. The Perkiomen scheme is not considered by him for want of the necessary data. Nothing, therefore, appeared to be feasible, but to continue to use the Schuylkill water. The question of raising it into reservoirs by water-power is answered negatively, on the ground that it would require an extensive and costly system of compensating reserveirs and dams, and, quoting from the report to the Reading Railroad Company, by James F. Smith, in 1874, he concludes that, taking a most favorable view, the largest available amount thus to be secured would be 100 million gallons per day. Steam power is therefore recommended as the most economical means for increasing the water supply for the immediate future. He urges a more careful study, however, into the best scheme for a more distant period.

The same report contains the results of a chemical analysis of the Schuylkill water and notes on its pollution, by Dr. Charles M. Cresson. His conclusions are, that the Schylkill water would be sufficiently good for the city if the sewage entering below Flat Rock Dam were intercepted, and the foulest sewage entering above it purified, before draining into the river.

In 1875, a memorial was presented to Councils by James Haworth on supplying the City of Philadelphia with water from the Schuylkill and Wissahickon by water-power. It was accompanied by a paper from J. W. Nystrom, Mechanical Engineer, supporting the view that a judicious employment of water-power would render steam unnecessary for supplying the city with water.

From the variety of opinions entertained on this subject, and from the difficulties presented in clearly viewing the proper plan for the future water supply, and therefore building present works in conformity thereto, but more especially from the urgent necessity of guarding against a water famine during the time of the International Exhibition, a Commission of Experts was appointed by the Mayor, in 1875, consisting of W. Milnor Roberts, William J. McAlpine, J. W, Adams, W. E. Morris, Solomon W. Roberts, and William H. McFadden, Chief Engineer of the Water Department, to whom the entire subject of the present and future supply was referred.

As to the latter question, which alone concerns us here, the report of this Commission was unsatisfactory, from want of a comprehensive view of the question and a positive expression of opinion, due, no doubt, to the magnitude of the subject, and to the insufficient funds

and time available for making the necessary inquiries. There was an absence of any valuable suggestions or of weighty arguments in any one direction which would more clearly indicate the proper future source of supply for the city. The Perkiomen scheme was rather favorably regarded by several members of the Commission, but, in the absence of more complete surveys and other investigations, they did not feel justified in recommending it.

The following propositions are discussed in the Report of the Commission of 1875:

1. *Increase of minimum flow of the Schuylkill river by storing storm water in impounding reservoirs.*—This scheme, if the reservoirs are formed by the river itself, would be much more expensive than raising the additional water by steam at Fairmount, and is, therefore, not recommended. If impounding reservoirs are built in the Perkiomen Valley, by means of a dam at Zieglersville, which would permit the storage of more than 20,000 million gallons, the minimum flow of the Schuylkill for a period of eighty days could be doubled.

2. *Pumping with the power of Flat Rock Dam.*—The Commission presents this idea with a favorable opinion, as far as cost is concerned, and recommends it as worthy of further consideration. The increase of supply from this source is estimated at 20 million gallons per day for three months, and 27 millions per day for the remaining nine months.

3. *Prevention of the pollution of the water pumped from the Fairmount Pool.*—An intercepting sewer, as recommended by the Special Committee of the Park Commission in 1867, is pronounced to be the most effectual remedy hitherto advocated. The plan of carrying the water of Flat Rock Pool to the Fairmount pumps is also mentioned, but no decided recommendation is made in either case.

4. *Gravity supply from the Delaware Water Gap.*—From the fact that the cost of a supply from this source would be, according to the estimates of the Commission, not less than $30,000,000, the adoption of this scheme is deemed inexpedient.

5. *New Hope Projects.*—The proposition to obtain water from the Delaware at this point likewise meets with disfavor on account of its expense, the cost of two alternate schemes being estimated at $23,000,000 and $22,500,000 respectively, including capitalized cost of pumping, to supply only 75 million gallons per day. One plan is to raise water at New Hope by steam power, and thence carry it to the city in

a high-level conduit. The other is to purchase the Delaware Division of the Navigation Company's Canal, change it to a supply canal, construct nine miles of new canal and seven miles of conduit, besides pumping the water by steam power at or near Lardner's Point.

6. *Scudder's Falls Project.*—The scheme to take the Delaware water at this point, situated two and one-half miles above Trenton, is likewise discarded on account of its cost, which is estimated at $21,500,-000.

It requires the purchase of the Trenton Water Power, the erection of a low dam, the building of twenty-four miles of a large supply canal and seven miles of conduit, besides pumping the water by steam power at or near Lardner's Point.

7. *Gravity Supply from the Perkiomen.*—This project is treated more at length, as it was considered the only reasonably practical plan on the score of economy. Its cost for the delivery of 100 million gallons daily is estimated at $10,000,000; for the delivery of 200 million gallons daily, at $12,000,000.

The water shed is stated to be free now and likely to remain free from causes of pollution. A suitable site for building a safe dam exists and at a point where a sufficient quantity of water can be impounded. It is assumed that fifty per cent. of the rainfall would flow into the reservoir, from which it is calculated that two million people could be supplied with eighty gallons each per day.

Considering the objections to this scheme, the Commission conlude that gravity works should not be constructed unless demanded for the purpose of obtaining a purer and better water, or unless the time is near at hand when the cost of gravity would be less than by any other means. To ascertain this relation with the required degree of exactness, the Commission recommends thorough and careful surveys for an accurate map, and estimates made in detail.

8. *Artesian Wells.*—The project of supplying the city from deep wells is laid aside on the ground that there is no probability that an adequate supply for the general use of the city could be obtained in that manner; and if there were, such plans are attended with great expense and extreme uncertainty, and in every case are more or less experimental.

No action was taken by Councils on the above recommendations concerning a thorough investigation of the Perkiomen, or of any other scheme.

Dr. McFadden, Chief Engineer of the Water Department, in his annual reports to Councils for the years 1877, 1878, and 1879, calls attention to this matter, and urges a study into a plan for an adequate future supply.

Mr. Birkinbine again contributes some information on the question, in several papers read before the Franklin Institute; one in March, 1876, on the Rainfall in the Basin of the Schuylkill River, containing valuable and interesting data; another in May of the same year, on the Relation between the Rainfall in the Schuylkill Basin and the Water discharged at Fairmount.

In May and July of the year 1878, Mr. Birkinbine gives, in the FRANKLIN INSTITUTE JOURNAL, an extended discourse on the future water supply of the city. He examines into the various schemes that have been heretofore proposed, and concludes, as formerly, by recommending the Perkiomen gravity scheme as the best and most economical one.

In May, 1879, Mr. Charles G. Darrach read a paper before the Engineers' Club of Philadelphia on the same subject. He endeavored to show that a supply by pumping is more economical than by gravity, until the quantity of water needed is 150,000,000 gallons per day, or about the year 1950. In discussing the gravity schemes, he favors the Perkiomen, but instead of a dam at Schwenksville, as proposed by Mr. Birkinbine, he recommends an intercepting canal built around what would have been the edge of the lake, with dams on the cross valleys, to avoid the high dam at Schwenksville and the consequent flooding of the populated part of the valley.

In October, 1879, the future water supply of the city is discussed by Mr. James F. Smith, Chief Engineer of the Schuylkill canals. He states that "the Perkiomen creek and its tributaries form the source from which the water for Philadelphia must eventually be brought, and gravity must be the mode of its conveyance." He criticises Mr. Birkinbine's plan, however, to the effect that the surface water at the proposed site for the reservoirs would be too low, except for the supply of the East Park Reservoir and the basins below it. The location of Mr. Smith's line begins at Green Lane, where a storage reservoir is proposed, and extends from there at an elevation of 273 feet above city datum to a terminal basin situated in the city at an elevation of 249 feet.

In the following year a pamphlet was again issued by Mr. James

Haworth, a citizen of Philadelphia, containing a report on the Philadelphia Water Supply, made to him at his request in 1878, by a Commission consisting of Messrs. J. W. Nystrom, W. Barnet Le Van and William Dennison. The document was entirely of a private nature, and set forth the opinion that, with the aid of proper impounding dams, the entire supply for Philadelphia could be furnished for nearly a century to come by the water-power of the Schuylkill River below Roxborough pool.

In June, 1882, the Mayor was again authorized by Councils to appoint a Board of Experts, to report, among other matters, on what should be done for the future water supply of the city. The Commission consisted of Messrs. E. S. Chesbrough, J. Vaughan Merrick and Frederick Graff, in conjunction with the Chief Engineer, and their report is of too recent a date to need more than a reference. They found it impracticable at the time to reach definite conclusions on the question, for the want of sufficient surveys and other data.

The present investigation, finally, is to furnish the information which, as the foregoing clearly shows, had already been urged a number of times, and was absolutely essential to a solution of the important questions at issue.

It has been your desire to have the investigation made as thorough as practicable, and that due consideration should be given to all possible solutions of the problem.

Your instructions were to have the preliminary surveys extensive and exact enough to render a re-survey of the same territory at some future time unnecessary, should slight variations in the conduit lines, or in the location of storage reservoirs, be proposed.

Accordingly, the following practicable schemes are being carefully investigated:

Water can be brought to the city from distant sources, and delivered at high elevations by gravity; or it can be brought from less distant points by gravity, but requiring some pumping; or, thirdly, it can be obtained from nearer but necessarily lower sources, and be delivered into reservoirs entirely by pumping.

SCHEMES FOR SUPPLIES BY GRAVITY.

To this class belongs the following:

1. *Delaware Water Gap.*—Although this source has been frequently

mentioned, it has always been considered of questionable value, since the distance from the city, which is over eighty miles, renders the scheme an expensive one. The cost, however, had never been carefully ascertained, and, as both the excellence and abundance of this supply are great, it was worth a thorough examination.

2. To bring the *Upper Schuylkill water* by gravity to the city will not be considered at present as it is at times charged with sulphuric acid from the coal districts at the point where it would have to be diverted, and it is likely to remain so.

3. *The Lehigh River at White Haven.*—This project, although mentioned, has met with even less favor than the Water Gap scheme, owing to the required length of conduit, which would be about one hundred miles. The unusual adaptability, however, of the Lehigh water-shed above White Haven, and of its eastern slope between this point and Mauch Chunk, to furnish potable water of an excellent quality, being free from inducements either for mining, farming or manufacture, on account of its geological features and high elevation, makes the same worthy of attention; not on account of a supposition that it might be best for any near future, for its cost at present precludes this, but on account of its possible connection with other schemes more favorable at present, but not sufficiently so to recommend their adoption without looking to the more distant future.

It seems clear that the Blue Mountains, between the Lehigh and Port Jervis, must ultimately be the source whence the water supply not only of Philadelphia, but of other cities between it and the mountains, will be brought. No inducement prevails to cultivate this territory, whilst the farming and mineral lands below it must in time increase the population and the industries to an extent that will make the pollution of the lower streams almost unavoidable.

4. *The Perkiomen Creek.*—Among all the projects for a future supply, this one has heretofore received the most attention. It was first suggested by Mr. Birkinbine, was favorably considered by the Commission of Engineers in 1875, and strongly advocated by Mr. James F. Smith, Chief Engineer of the Reading Canals.

It has, however, been impossible to give it legitimately an unqualified endorsement, for the reason that the necessary data for a full comprehension of the scheme were wanting. Moreover, there is a popular belief that the available quantity of water is insufficient, and that its quality is lacking in the necessary degree of excellence. To

definitely settle these points, a thorough investigation of this entire project, considered in all its bearings, was directed.

5. *The Neshaminy and Tohickon Creeks.*—I am not aware that these creeks have ever been proposed as available for the future water supply of the city; nor would they be worth considering as forming a scheme by themselves, because their combined water is less in quantity than the water from the Perkiomen basin, although the quality cannot differ very materially from it. But as the location of a conduit bringing this water to the city, would be identical with one bringing water from the upper Delaware, this scheme presents many advantages. It will be referred to more fully under the next head.

SCHEMES FOR SUPPLIES BY GRAVITY, SUPPLEMENTED BY PUMPING.

Among the projects grouped under the second class, namely, those by which water can be partially brought by gravity from less distant points, and partially pumped, we can mention the following, which must naturally belong to the Delaware watershed, as the purity of the Schuylkill water does not increase materially with its distance from the city:

1. *Delaware River at Scudder's Falls.*—This scheme was considered by the Commission of Engineers in 1875, and has already been outlined. Its cost removes it at present from the necessity of any serious consideration.

2. *Delaware River at New Hope.*—Two projects have been proposed to bring water from this point. They were both considered by the same Commission, and have also been already described.

3. *Delaware River at Point Pleasant.*—Above Point Pleasant the Delaware river falls about fifteen feet in two miles, therefore furnishing a considerable water power. Point Pleasant, furthermore is situated at the mouth of the Tohickon Creek, and on the only practicable conduit line to the Delaware Water Gap.

A high-level conduit therefore, starting at this point, and delivering at the city reservoirs by gravity, is a practicable scheme, possessing the following advantages: It would carry the waters from the Neshaminy and Tohickon watersheds, collected by storage dams, to the city by gravity. The deficiency could be more than supplied from the Delaware river at Point Pleasant by steam or even by water power.

The canal carries the low-water flow of the Lehigh river past this point, and therefore prevents its polluting the proposed supply. This conduit would eventually form a part of one to the Delaware Water Gap, being over one-third of its entire length.

This scheme has not been mentioned in any of the documents quoted, nor has it, to my knowledge, ever before been suggested.

SUPPLIES ENTIRELY BY PUMPING.

The projects of the third class contemplate the use of the water within the limits of the city, as at present, by pumping to the necessary elevations, either by water power or steam. Artesian wells are the only source of this kind other than the Delaware and Schuylkill rivers. With regard to these the following remarks may be made. There are localities in this city where a large amount of water could be obtained, apparently unpolluted by surface infiltrations. The probable quantity of this water could be approximated only by a very careful geological and topographical examination, probably not without the aid of borings. While it is extremely doubtful whether sufficient water could be found to supply more than a small portion of the city, it might yet be found practicable to a certain degree, in case of serious objections to all other schemes.

From the above it is evident that the extent of territory requiring investigation for the future water supply is very large, much more extensive, in fact, than the territory available for the same purposes near any of the other large cities of the Union. New York had little choice, and was naturally led to the impounding of the Croton water, Boston to several small streams lying northwest of it, Baltimore to the Gunpowder creek, Brooklyn to the small streams east of it, Cincinnati to the Ohio river, St. Louis and New Orleans to the Mississippi, and the Lake cities to the Lakes,—while Philadelphia commands a territory comprising almost the entire Delaware, Schuylkill, and Lehigh watersheds, with a number of small tributaries which may be used either independently or in connection with the main rivers. In weighing their relative advantages, it was decided, that at least the following schemes should be investigated, as they seemed to be the most practicable:

First, the Perkiomen project, in all its bearings, as it has heretofore been believed to offer the greatest advantages.

Secondly, a conduit line to the Delaware Water Gap, and the practicability of temporarily impounding the water along its course.

Thirdly, as the Perkiomen basin alone might be found to furnish an insufficient supply for a distant future, the Lehigh basin was to be examined to ascertain whether its water could be brought into the Perkiomen at a later time, in order to compare this combined project with the Delaware Water Gap scheme, which requires about the same length of conduit.

And fourthly, a sanitary survey of the Schuylkill valley was needed to show the nature and amount of the polluting elements, and the cost ascertained, as nearly as possible, of maintaining the purity of its water in the future, keeping in view an increasing development of the industries and growth of the population in the valley.

It was decided, therefore, to place two surveying parties in the field, one to work up the topographical features of the Perkiomen, the other those of the Delaware project, and a third party to take charge of the hydrographic work. In addition, a careful reconnaissance was to be made of the Lehigh water-shed, also a sanitary survey of the Schuylkill valley, and a geological survey of the respective water-sheds and of the territory over which the conduit lines were to pass.

The amount of work done by the different parties during the last year, classed as topographical, hydrographic, and miscellaneous, is as follows:

A TOPOGRAPHICAL WORK.

The information to be gained under this head was: First, the most feasible conduit routes; secondly, location for storage reservoirs; and thirdly, the physical, sanitary, and commercial features of the several water-sheds.

Under physical features were to be understood the general contour and elvation of the ground, the respective areas of woodland swamps, meadows and arable land under culvation and of the towns and villages.

Under commercial features were to be considered the questions of possible future developments on account of mineral agricultural, or other natural resources, or from being readily crossed by railroad lines.

Under sanitary features were to be classed the population, death or sick rate, the location and extent of mills, factories, slaughter-houses,

cemeteries sewage, etc., and the amount of refuse matter probably reaching the water courses.

The topography was taken with considerable care, especially for the conduits, so that a close location and estimate of the cost can be made. A preliminary inspection was first undertaken, with the assistance of the country maps and aneroid barometers, in order to avoid the running of useless lines. In considering the practicable routes, tunnels were not to be regarded as being very objectionable, as they give a safer and more durable conduit, with a tendency to lower the temperature of the water in summer, and to decrease the land damages. They save distance and increase the grade of the conduit, and the increased cost over open cutting is now much diminished by the improved appliances for tunneling.

The conduit lines were run at an elvation which would permit a discharge into the Cambria and Frankford reservoirs, or at 165 to 175 feet above city datum.

The transit lines were measured with steel tapes. The topography was filled in by triangulations, by gradienter and stadia measurements, by clinometer and slope rods. The main lines were very carefully leveled, after the method of double turning points, with very satisfactory results. Three check lines were leveled, completing circuits respectively of 97·23 miles, partially over a very rough country, with an error between the levels of the two parties of only ·997 ft., or ·0,102 ft. per mile; of 73·4 miles, with an error of only 0·473 ft., or 0·0,064 ft. per mile, and of 34·5 miles, with an error of only 0·07 foot, or 0·002 foot per mile.

During rainy days the time was occupied in calculating latitudes and departures for the plotting of the conduit lines, computing and checking bearings and distances, calculating elevations, inking field notes, cleaning and repairing tools.

(*a*.) *Perkiomen Party*, Mr. Harvey Linton, C. E., in charge.

This party began operations May 29, 1883, and remained in the field until December 17, 1883, or 175 working days, when the weather made it necessary to remove to the city and begin office work.

During this time 101·94 miles were measured with steel tape, of which 53·40 miles were for conduit lines; 325·89 miles were measured by stadia and gradienter; 200 miles of slopes were taken with clino-

meter; 3·566 vernier angles were turned; 5·719 magnetic bearings were taken with the transit; 204·44 miles were leveled with the "Y" level, and 504 bench marks were established.

The areas covered by the survey were about:

$11\frac{1}{2}$	square miles for conduit lines,
6	square miles for storage basins, and
90	square miles for general water-shed.
$107\frac{1}{2}$	square miles.

CORRESPONDENCE.

ELLIPTICITY OF PLANETS.

Editor of the Journal of the Franklin Institute:

In the number of your JOURNAL for July, 1884, I have noticed a criticism, by Prof. Chase, upon my paper on the Ellipticity of Planets, published May, 1884, in the same JOURNAL.

Perhaps my knowledge of mathematics and astronomy is too limited, or too antiquated, to enable me to fully understand Prof. Chase's harmonic system as applied to the determination of the Earth's actual oblateness; but there is surely one thing clear to me, that is, the Professor makes an unnecessary display of calculations to find a number (16·98), which he could obtain by simply taking the square root of another number he has already adopted. In fact, he accepts Newcomb's estimate of the ratio of centrifugal force to attraction at the equator, viz.: $1 \div 288{\cdot}4$; consequently $\sqrt{288{\cdot}4} = 16{\cdot}98$ must represent precisely the number of times the Earth's velocity of rotation should be increased in order that the centrifugal and centripetal forces at the equator balance each other. How the Professor can imply from this, that $1 \div 288{\cdot}4$ must be the actual oblateness of the earth I am at a loss to understand.

As to the meaning of his last paragraph where speaks of connections of Neptune with the Earth, etc., I invite the Professor to explain by what reason and how the number 16·98 connects the planet Neptune with the Earth, and what the ratio $329196 \div 16{\cdot}98$ represents. In this connection he may also please to explain why he has changed the number 327994 given in Sun-Earth Balance (this JOURNAL, May, 1884), for the other, 329196.

L. D'AURIA.

Editor of the Journal of the Franklin Institute:

I send you the records of the test of a Turbine Wheel, recently made by me, at the John P. King Cotton Mill, at Augusta, Ga., which are interesting, not only from the excellent result attained, but from the great amount of power controlled by the Prony-brake, it being one of the heaviest tests ever made, if not the heaviest, the only other one of such magnitude, to my

knowledge, being that of the Boyden Turbine at the Atlantic Mills, at Lawrence, Mass., made by Mr. Boyden himself in 1851.

The wheel at Augusta, was what is known as a "Geyelin Jouval," built by Messrs. R. D. Wood & Co., of Philadelphia, 7 feet exterior diameter, with 36 buckets, each $13 \cdot 875 \times 1 \cdot 7217$ inches, giving a total area of discharge of 860 square inches, or 5·972 square feet

The friction pulley and brake were constructed by Messrs. R. D. Wood & Co., from my directions, and were as follows: Pulley, 7 feet diameter and 2 feet face, with exterior flanges 1 inch thick and 2 inches deep.

The brake was a hoop of $\frac{3}{8}$ inch boiler iron, in two pieces, lined with segments made from 4 inch oak plank, placed vertically, so as to bring the grain of the wood at right angles to the diameter of motion of the pulley, and grooved "herring-boned," on the interior surface, to admit of the passage of the lubricant, which was soap, diluted to about the consistency of cream, and allowed to flow in a stream of the size of a quill, from two feeders, placed 18 inches, and 4 feet, from the opening in the brakes, where the tightening screw drew it together, and where there was about 18 inches free space, into which a jet of water was thrown from a hydrant in the mill yard, through a fan-shaped nozzle made of lead pipe, $2\frac{1}{2}$ inches long, and $\frac{1}{4}$ inch wide. This also served to cool the pulley, as well as aid in the lubrication. The band was attached at one end to an oak timber 17 feet long and 18 inches square, which formed the first lever, and to the other end of the band was attached by a link, a screw 2 inches diameter, which passed through this timber, and was operated upon by a gear 18 inches diameter, having the nut for the screw cut in its hub. This was driven by a pinion 6 inches diameter, the shaft of which was supported in bearings on top of the timber, and had on it a hand-wheel 3 feet diameter for the operator.

A link at the end of this brake lever, connected it with a bent lever, or scale beam, having its horizontal arm 12 feet in length, and its vertical one 6 feet. The fulcrum for the lever, and the pivot from which the scale pan was hung, were of $1\frac{3}{8}$ inches steel, knife edged and hardened, and worked in iron sockets. A hydraulic regulator, or "dash-pot," 18 inches deep and 18 inches diameter, was attached to the scale beam, 10 feet from the fulcrum, or so as just to clear the weights.

The scale beam was balanced, by a weight, attached to a chain, at the extreme outer end, and passing over a grooved pulley, 12 inches diameter, and the weight required to balance it, when the "dash-pot" was filled with water, was $292\frac{1}{2}$ pounds.

The water was measured by a hook gauge, placed at a weir 19 feet long, set in the tail race, about 100 feet below the wheels, so as to get a smooth and steady flow over the weir, and its upper edge was 4 feet 6 inches above the bottom of the race at that point.

The weir crest and sides were made of Georgia pine, having straight edges, $\frac{1}{4}$ inch thick, and beveled away "down stream," at an angle of 45°.

I was aided in the weir observations by Mr. Davidson, the City Engineer of Augusta, who was kind enough to volunteer his services, and as the water was very muddy, Mr. A. L. Olmstead, C. E., of Lockport, New York, who enlarged and rebuilt the canal system at Augusta, very kindly determined the weight of a cubic foot of it for me by actual experiment, finding it to be 62·95 pounds.

The point of attachment of the brake lever to the scale-beam was 14 feet 9½ inches from the centre of the wheel shaft, giving a circumference of 92·9388 feet. This was doubled by the scale-beam, making the actual circumference of the brake circle = 185·8776 feet, which is the basis of the calculation. The intended velocity of wheel was 76·24 revolutions per minute, and 76·19 revolutions was accepted as decisive; this speed was counted by a worm shaft, tapped into the wheel shaft, with a gear and striker which rang a bell at every 100 revolutions. The head acting on which was determined by a float-can of galvanized iron, in a box in pit, with holes in bottom of box, the can supporting a light rod, graduated from line of flotation and parallel to a glass tube in which a column of water showed the height in flumes, so that the acting head was read at once.

SAMUEL WEBBER.

Test of "Geyelin Jouval" Turbine, at Augusta, Ga., May 3d, 1884, by Samuel Webber, C. E.

	No. 1.	No. 2.	No. 3.	No. 4.
Head of water acting on wheel	29·30 ft.	29·30 ft.	29·30 ft.	29·30 ft.
Weight of water per cubic foot	62·95 lbs.	62·95 lbs.	62·95 lbs.	62·95 lbs.
Weight in scale-pan	962·75 "	1,057 lbs.	1,082·5 "	1,092·5 "
Number of revolutions per minute	88·23	79·65	77·42	76·19
Whole number revolutions made	200	200	200	400
Horse-power attained by wheel	468·52	471·01	472·08	468·85
Corrected height of water on weir	Water not taken, trial only for experiment to attain proper velocity, which was fixed at 76·24 revolutions per minute.	Water not taken, trial only for experiment to attain proper velocity, which was fixed at 76·24 revolutions per minute.	1·9944 ft.	1·9853 ft.
Corrected height of waste water (⅔ deducted from this)			0·281	0·281
Cubic feet of water passing weir per minute			10190·59 ft.	10018·50 ft.
Horse-power of water, after deducting waste from of two other wheels in same pit, or ⅔ of total leakage, and correcting for velocity of approach to weir = 0·852 ft. per sec			569·57	559·95
Coefficient of useful effect of wheel			·8288	·8367
Proportionate effect of wheel in horse-power at 32 ft. head			....	534·78
Coefficient of discharge as compared with theoretical velocity			0·655	0·644
Ratio velocity of wheel at centre of bucket, to theoretical velocity of discharge; the velocity of wheel being intended to be that due to 24 ft. head			0·545	0.536
Ratio velocity wheel, to that of water			0·846	0·8427

Guaranteed power of wheel, 32 ft. head, 475 horse-power.
" efficiency, 80 per cent.
" discharge, 9,800 cubic feet.

Book Notices.

On an Unsymmetrical Law of Error in the Position of a Point in Space. By E. L. De Forest, Watertown, Conn.

The readers of Professor Kendrick's late valuable journal, *The Analyst*, will remember several interesting articles by Mr. De Forest, one of which was "On an Unsymmetrical Probability Curve." In the transactions of the Connecticut Academy, Vol. vi, March, 1884, the discussion is extended so as to cover a similar law of error in the position of a point in space. It suggests several points which are well worth the consideration of mathematicians, a few of which we will enumerate:

1. There is a symmetry, even in the lack of symmetry, so that the evidence of law is as striking as ever.

2. The lack of symmetry is due to the insertion of a subsidiary datum. The result, therefore, increases the evidence that mathematics, like ordinary logic, can only take out what it has first put in.

3. The discussion may help even some mathematicians, to more correct ideas of what is meant by probable error, and what is really involved in the expression.

4. It may help to correct the erorneous impression, into which even so eminent a mathematician as the late Professor Clifford fell, that mathematical certainty can never be other than empirical and that we cannot assert, as an absolute truth, that two parallel lines will never meet.

5. It may also help to correct the opposite error, that it is impossible to arrive at different conclusions, in discussing mathematical questions from opposite points. Take, for instance, the following illustration:

Many readers of this journal have listened to some recent rather acrimonious discussions, among different mathematical members of the Institute, as to the practical merits of an apparatus devised for the same purpose as the bathometer, of Siemens. Different members of the committee, to which the apparatus was submitted for examination, agreed in believing that it would not work satisfactorily; but by mathematical discussions of special points they arrived at conclusions which they were unable to reconcile. If only one of the points which was discussed had been suggested, it is probable that all would have accepted the conclusion to which it led, but neither of them seems to have considered that a complete and correct decision could only be reached, after all the points had been duly weighed and the results which would harmonize them all had been satisfactorily ascertained.

C.

A New System of Laying Out Railway Turnouts Instantly, by Inspection from Tables. By Jacob M. Clark. New York: D. Van Nostrand, 1884.

This very neat little volume gives, first, a table (I) of switch-lengths corresponding to different throws, and of frog distances and frog angles corresponding to different gauges and to different clear distances between two lines of a double track; for turnouts from a straight track. The chord-

deflection angles of the turnout curves, range from 1° to 50°. Tables II and III give corrective quantities to be added to, or subtracted from, the frog distances and frog angles of table I, in cases where the main line is curved. The remainder of the volume explains the principles of the method, and the manner of using the tables.

By the author's prefatory admission, this system of finding distances and angles in the case of turnouts from curves, appears to have been first made public by Mr. E. A. Gieseler in 1878, in a tract to which the writer was indebted for its introduction into the *Civil Engineer's Pocket Book* of last year, to which Mr. Clark kindly refers.

The fifteen illustrations are conveniently printed upon three folded inset sheets at the end of the book, so that any figure may be brought opposite to any page upon which it is referred to. It is to be regretted that the same regard for the reader's convenience did not lead to greater generosity in the size and lettering of the figures, and to greater exactness in the wording of the text. As it is, some of the propositions require, for their complete comprehension, a second reading; which, however, in view of the beautifully clear typography, can scarcely be complained of as a hardship.

The book forms a useful exposition, (probably the fullest that has yet appeared) of this method of laying out turnouts from curved main lines.

J. C. T., Jr.

Tables for Calculating the Cubic Contents of Excavations and Embankments, by an Improved Method of Diagonals and Side Triangles. By John R. Hudson, C E. New York: John Wiley & Sons, 1884.

The author's claim of novelty for this method, seems to be well founded. Like Henck's, it treats of each half of a hundred foot section of excavation, or of embankment, divided longitudinally by the central vertical plane of the road, as being composed of pyramids; but while Henck divides it into *three* pyramids, *all* quadrangular, and having a *common apex* at one end of the section, Mr. Hudson divides it into *four* pyramids. Two of these have *triangular* bases, a common apex, and one edge in common; while each of the other two has, for its quadrangular base, the half section at one of the stations, and, for its apex, one of the corners of the half cross section at the opposite station. The sum of the volumes of the two triangular pyramids is shown to be equal to that of an imaginary quadrangular prism, of length=one-third the length of the 100 feet section, and having its base=the area of an imaginary *half* cross section (inadvertently referred to as "a cross section") with a center height=the center height at one station, and a side height=the side height at the other station. The novelty of the method consists partly in the introduction and use of this prism, and partly in the employment of tables of contents, in cubic yards, of prisms 100 feet long, whose cross sections (uniform throughout each prism) are the "side triangles" (in the actual cross sections) above and below the ground surface of a *level* section of the same center height.

The "table of side triangles," for each given width of roadway and given side slope, faces the corresponding table of contents for level sections. These last do not differ from those in common use.

The operation consists in taking from these tables "of level sections" and "of side triangles," (by simple inspection and addition) the contents, in cubic yards, of four prisms, each 100 feet long, and each of uniform cross section throughout; the several cross sections being, respectively, that at one station, that at the other station, that of the imaginary prism to the right of the center plane, and that of the corresponding prism to the left of said plane. These four contents are then added together, and their sum divided by 3, the quotient giving the contents of the 100 feet station, in cubic yards.

The "distances out" are not required to be given; nor are any drawings of the end or middle cross sections, or calculations of their areas, required to be made. The author assures us, speaking no doubt from experience, that "with a little practice this method will be found rapid." That this should be the case will be readily understood when it is remembered, that, as already stated, the entire process consists in inspection of tables, addition, and division by 3. The simplicity of the operation tends, also, of course, to freedom from liability to error.

In its application, the method appears to be limited to "three level sections." For five level sections, and for side hill work, etc., it would have to be supplemented by some of the methods already in use.

J. C. T., Jr.

Selective Absorption of Solar Energy.—In the year 1800, Sir Wm. Herschel published, in the Philosophical Transactions, his remarkable researches upon obscure heat, which led him to conclude that heat is distinct from light. Subsequent writers taught that each solar ray possesses three qualities: heat, light and chemical action. Some modern investigators have concluded that the same æthereal undulations produce either heat, light or actinism, according to the absorbing nature of the substance which receives them. Dr. J. W. Draper observed that the maximum of heat was not necessarily found, in all cases, below the red portion of the spectrum. He concluded that in a normal spectrum it should be found in the orange, and proposed experiments upon spectra of diffraction, which he had no instruments delicate enough to carry out. Prof. Langley's invention of the bolometer has enabled him to obtain the following results: 1. In the diffraction spectrum, the maximum energy is found above the red and near the yellow ray. The situation of this point varies with the altitude of the sun, between a wave length of 0·55, on a clear day at noon, and 0·65, or more towards evening. 2. On comparing the ordinates for high and low altitudes of the sun in different parts of the spectrum, they are found to increase toward the ultra violet and decrease toward the infra red, appearing to follow a simple law of decreasing when the length of the wave increases. 3. By employing the logarithmic formula, which is legitimately applicable to homogeneous waves, we can pass from the curve for the interior to the curve for the exterior of the atmosphere; in other words, we can virtually transport our station of observation to a point beyond the

atmosphere, and determine the distribution of solar heat before it has been effected by the irregular absorbent action of the air. We then find that the maximum heat is between the wave lengths of 0·50 and 0·55, or nearer the green than the yellow. The measurements make it probable that the sun would appear distinctly blue to the naked eye, if it were not for the atmosphere. Our white light is not then the sum of all the radiations, but only of a part of the visible radiations. By measuring the area of the curves without and within the atmosphere, we can obtain a value for the solar constant by an entirely new method. The observations of Messrs. Pouillet and Violle gave, respectively, for this constant, 1·7 and 2·5 calories; Langley's preliminary observations gave 2·84, and he thinks that the true value is probably very little, if any, below three calories. 4. These observations increase the reasons for thinking that all the energy in any ray could be manifested as heat if it was received in a suitable medium.—*Ann. de Chim. et de Phys.*, Aug., 1883. C.

Use of Oxygen as a Refrigerant.—When liquefied in large quantities and suddenly evaporated by the instantaneous removal of pressure, oxygen does not solidify, like carbonic acid, but it leaves a crystalline deposit at the bottom of the apparatus, which soon disappears when the temperature begins to rise. Wroblewski is not yet able to say whether the crystals are composed of oxygen alone, or whether they come either wholly or in part from possible impurities. In refrigerating glass tubes the thin layer of this opaque deposit often interferes seriously with the readiness of observation. Another circumstance which adds to the difficulty of using liquid oxygen as a refrigerant is the necessity of keeping it in closed apparatus of great strength. It has not yet been obtained in a permanent form under the pressure of a single atmosphere. The containing apparatus being constructed partly of glass, there is continual danger of a serious explosion, so that experiments should be made with the protection of a strong mask for the face.—*Les Mondes*, Jan. 26, 1884. C.

Chestnut Timber.—There is a common tradition that the timbers of old churches were made of chestnut wood. If this were the case, chestnut trees must once have been common in countries where now they are extremely rare. A skillful chemist, M. Payen, has procured a large number of specimens of timber from the old churches and buildings in Paris, which he has subjected to careful examination, and he believes that none of them were chestnut. If letters are drawn upon oak and chestnut planks, by means of pure sulphate of iron dissolved in distilled water, the characters appear at once, in black, upon the oak and in deep violet upon the chestnut. Ammonia produces a red color, of short duration, upon the chestnut, paler and less distinct upon the oak. All the French and American varieties of oak show very distinctly, on their transverse sections, medullary rays crossing the woody fibres from the centre across the circumference. Chestnut timber possesses only concentric layers.—*La Science pour Tous; Les Mondes*, Feb. 9, 1884. C.

Monthly Periodicity of Auroras.—Terby has endeavored to show that there is a monthly period of the Northern auroras. He seeks to ex-

plain it by the appearance and return of the same solar spots towards the earth after a revolution of the sun upon its axis. Quetelot, the brothers Tromholt, and others, have suspected a monthly period, but their investigations have not proved decisive. On examining the photographs of the Kew Observatory, for four years, Terby found numerous coincidences between the passage of spots or groups of spots over the central meridian of the sun and auroral phenomena which were visible in Belgium on the same days. Montiguy, in his report on Terby's paper, is unwilling to admit all of his conclusions, but he thinks the subject of sufficient importance for further careful investigation.—*Bull. de l'Acad. de Belg.*, July 7, 1883. C.

RELATIONS OF HYDROGEN.—T. Stacewicks (*Pharmaceutische Zeitschrift für Russland*, 1884, p. 33) compares the specific heats and densities of different substances, and comes to the strange conclusion that magnetism, electricity, heat and light are nothing else than rarefied hydrogen.—*Dingler's Journal*, April 2, 1884. C.

MOSS BOARD.—The old moss, which forms beds of more than a foot in thickness, is found in prodigious masses at various places in Norway and Sweden. Although half decomposed, it constitutes an excellent material for the manufacture of paper; specimens of which have already been offered for sale, Cardboard has been manufactured from it, some leaves of which are 20 millimetres (·787 inch) in thickness. It is hard as wo d and can be very easily dyed and polished. It is likely that this product can be substituted for wood with great advantage in many cases; for it possesses all of its good qualities and has none of its defects; it neither splits nor warps. It can therefore be employed in the manufacture of doors and windows, cornices and many ornaments. Under the hydraulic press it can receive a consistency and a power of resistance far superior to those of straw board.—*Moniteur Industriel; Chron. Industr.*, March 30, 1884. C.

USE OF CASEINE FOR SIZING.—All albuminoid substances may be used for paper sizing; but the albumen which is extracted from eggs or from blood is too expensive for general use. M. Muth, of Carlsruhe, substitutes for egg-albumen the caseine of milk, which has identically the same chemical composition. The caseine is but slightly soluble in pure water; but it may be wholly dissolved in water that is slightly alkaline; especially in very dilute aqua ammonia.—*Chron. Industr.*, March 30, 1844. C.

PHOSPHORIZING BRASS.—J. Whiting places bronze or brass wire in a solution of from ½ to 5 per cent. of phosphorus in ether, bi-sulphide of carbon, or olive oil; 5 to 10 per cent. of sulphuric acid; and 85 to 95 per cent. of water, until the metal begins to take up the phosphorus. The wire is then drawn to one number finer and placed in a closed retort, with a thin layer of phosphorus, so that the phosphorus vapor may spread over the surface of the wire. The wire is then packed in charcoal, which is kindled, and after proper annealing the wire can again be drawn to a finer number. This process is repeated until the desired fineness is obtained. Wire thus phosphorized is very tough, takes a high polish, and is not easily corroded.—*Dingler's Journal*, Feb. 27, 1884. C.

Franklin Institute.

THE INTERNATIONAL ELECTRICAL EXHIBITION.

The illustration which appears as the frontispiece to this issue of the JOURNAL, gives a very fair representation of the buildings which will be occupied by the Institute for the purposes of the "International Electrical Exhibition," and for which extensive preparations have been, and are being made by the Committee on Exhibitions of the Board of Managers.

Since the appearance of the communication in the JOURNAL for September, 1883, the work has assumed such shape, that the Committee may at this time safely give the assurance that the Exhibition will be a large and representative one.

The demands for space by intending exhibitors some months ago, were already so large, that the Committee foresaw the necessity of providing for increasing the exhibition space at its disposal; and, on representing the case to the officers of the Pennsylvania Railroad Company, the old passenger depot adjoining the Exhibition building proper, was placed at the service of the Institute. This liberal action of the Company has nearly doubled the amount of space available for exhibition purposes, and has relieved the Committe of what threatened to be a serious embarassment.

The Committee charged with the duty of preparing a schedule of the tests to be conducted of the machinery and appliances on exhibition, has elaborated a code, and has invited a large number of eminent specialists in electricity and mechanics to co-operate in the work. A Superintendent and an Electrician have been appointed by the Board, and the work of allotting space is actively going on. The main building is practically finished, and the foundations for boilers and engines are being prepared; from all of which it will be inferred that the preparations for the coming event are in a forward state.

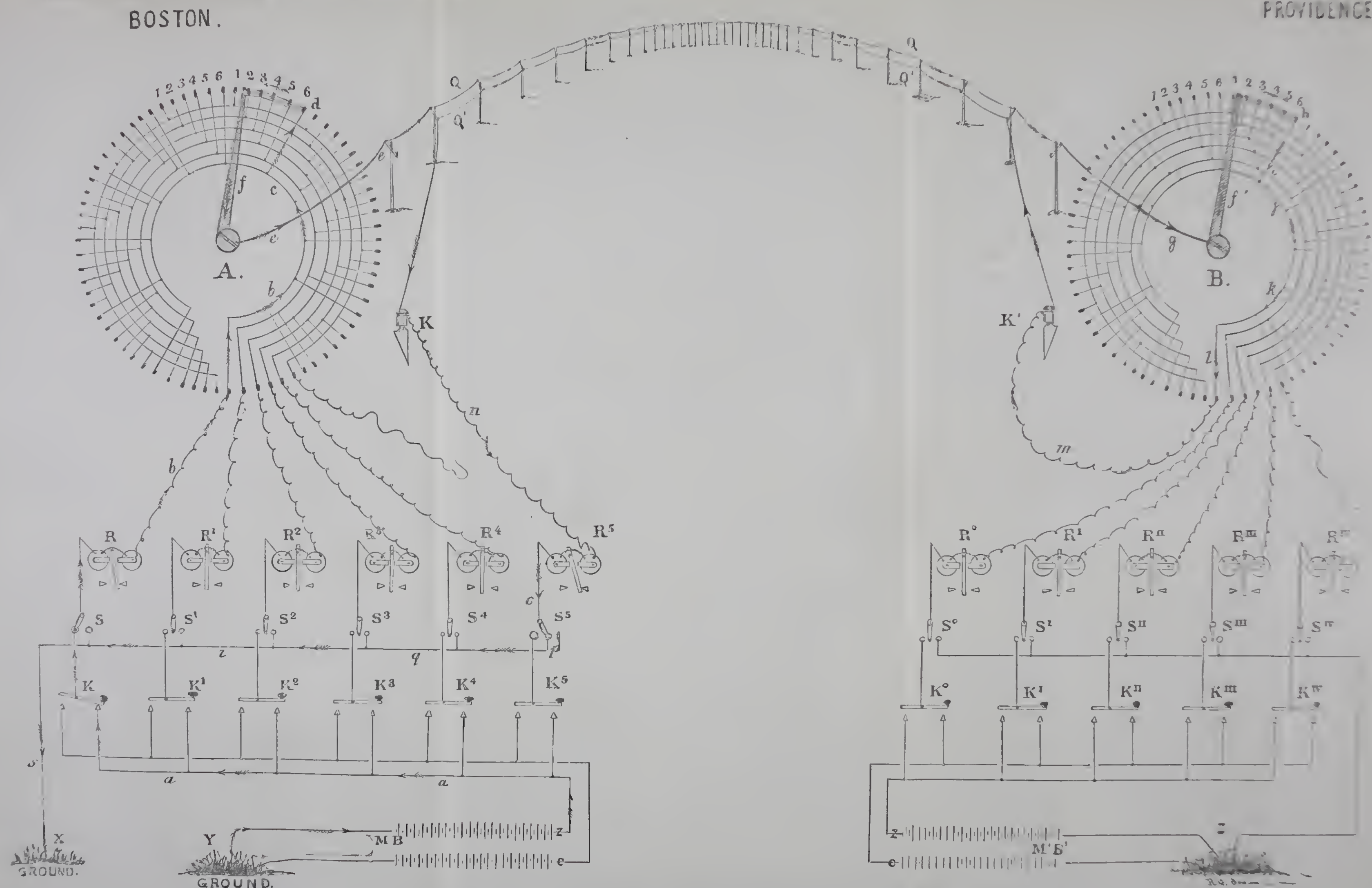
The Committees having charge of the preparation of a bibliographical and historical collection, have met with much success in their work, and in both of these departments the exhibits promise to be large and valuable.

Early in the history of the Electrical Exhibition, a movement was started, looking to the holding of a National Congress of Electricians to convene in Philadelphia during the time of holding the Exhibition, and asking Congress to appropriate a sum of money from the National Treasury to defray the expense of the work. This project has been successfully carried out, and a commission entrusted with the task of organizing the Congress will shortly be named by the President of the United States.

It thus appears to be assured that the opportunity of utilizing the scientific forces which will be gathered together in Philadelphia at the time of the Exhibition will be improved. W.

[Journal of the Franklin Institute, Vol. CXVIII., September, 1884.]

Houston



SYNCHRONOUS-MULTIPLEX TELEGRAPHY IN ACTUAL PRACTICE.

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SYNCHRONOUS-MULTIPLEX TELEGRAPHY IN ACTUAL PRACTICE.

By PROF. EDWIN J. HOUSTON.

It will interest the public generally to learn that Mr. Patrick B. Delany has successfully put into active operation his synchronous-multiplex system of telegraphy between the cities of Boston and Providence, R. I., a distance of about fifty miles.

The line is constructed of number six galvanized iron wire. For the purpose of securing one wire for operation in case of the accidental interruption of the other, and with a view to extension of the system, two wires have been strung. It will of course be understood that each of these wires is intended for separate use under any of the divisions of which the synchronous-multiplex system is capable; viz., any number from a single circuit up to seventy-two separate and distinct circuits over one and the same wire; or, as these are generally used in actual practice, into six fast, or twelve slower Morse circuits; or into thirty-six, or seventy-two printing circuits.

When the possibilities of the Delany synchronous-multiplex system were first brought before the public, grave doubts were expressed by some, if not by a majority of the leading electricians of the country, as to the possibilities of its actual operation under the conditions of commercial practice. Many believed that although it might be operative under the conditions of an artificial line, established in the laboratory

by means of resistance coils and condensers, that when put into actual operation the conditions necessary for continuous working could not be maintained.

It may be interesting, briefly to review some of the many objections that at the outset were urged against the practical workings of the system. As is, of course, understood, the possibility of the successful operation of the synchronous-multiplex system is dependent on the continuance of the synchronous rotation of the distributing wheel at the transmitting and receiving ends of the line. In Mr. Delany's system, as the reader will probably recall, the synchronous rotation of the discs at each end of the line is maintained by timed, electrical impulses, thrown into electro-magnetic motor devices, by the vibration of similar tuning forks placed one at each end of the line. Now it was urged, and perhaps with some show of reason, that although it might be possible to maintain the synchronous vibration of these forks in a room, where any necessary adjustment of their rate of vibration could be made by an attendant, yet it would clearly be impossible to maintain such synchronous vibration at stations widely separated from each other, since the mere difference of temperature at the two stations would, unless automatically compensated for, be sufficient to throw the two forks out of synchronism.

Another difficulty that in the opinion of many presented an insuperable obstacle to the practical application of the system was the static charge of the line. It was thought that the line could not in practice be discharged with sufficient rapidity to permit the transference thereon of the numerous separate and distinct electrical impulses necessary in this system. It was feared that before the line could be freed from the charge given it by one impulse, another would be sent over it by the distributor, and that these two would necessarily interfere with each other.

These, and many other difficulties, were urged as necessarily fatal to the success of the system. It is to the credit of Mr. Delany that he has so ingeniously met and overcome all these difficulties, and has established his invention on a commercial basis. But let the actual facts of the case speak for themselves.

Dividing one of the wires between Boston and Providence into six separate circuits, it was worked for long periods at the rate of forty words per minute on each of the circuits so established. Dividing the

line into twelve Morse circuits, it was similarly worked at the rate of twenty words per minute on each line.

Thirty-six printing circuits have been worked between the two cities at the rate of from four to five words per minute; while seventy-two printing circuits were similarly worked at the rate of from two to three words per minute.

The circuits above referred to have all been operated in one and the same direction at the same time, or have been operated one-half in one direction and the remaining half in the opposite direction, or other combinations of the same number of separate and distinct circuits have been employed.

In order to practically note the effect produced by increasing the length of the line, the two wires were joined together at Providence, thus providing one continuous circuit from Boston to Providence and back again to Boston, with separate grounds at each end in Boston. Over this double distance of about one hundred miles, the circuits were operated, as above mentioned, without any diminution of speed.

Further experiments introducing artificial resistances of 3,000 ohms, or an equivalent of about 300 miles of line, and of $2\frac{1}{2}$ microfarads of static charge, demonstrated the entire success of the system, under these conditions, with only a slightly diminished speed.

Employing the line as a sextuplex, on Saturday, July 12, 1884, 1,000 words were transmitted over one of the sextuplex circuits Boston via Providence to Boston, and received, at the rate of thirty-five words per minute, by sound, by Morse operators, who had never seen the system before that week.

Employing the line as a duodeciplex, 1,100 words were transmitted over one of the twelve lines so provided, at the rate of twenty words per minute and perfectly received by sound. An increase in the resistance of the line of 9,000 ohms, in addition to the normal resistance of the 100 miles of wire, in the two wires joined as one, did not affect the synchronism, or prevent the perfect transmission of the messages.

The synchronous-multiplex system of telegraphy has now been in actual operation between Boston and Providence, under the various conditions above mentioned, during the last thirty days. During that entire time, the synchronism has been maintained between the Boston and the Providence instruments without ten minutes interruption, excepting, of course, when the instruments were purposely stopped, or were

interrupted for the purposes of experiment, or were disturbed by crosses or breaks in the wires of the main line. When thrown out of synchronism by any of these causes, the instruments at the ends of the line, in all cases, came automatically into synchronism within one and a half minutes, without the intervention of the operator at either the Boston or the Providence end of the line.

Mr. Delany has availed himself of the opportunity which the additional line afforded him of trying practically things for which he believed his system adapted, and for which he originally intended it. One of the many purposes to which he showed the applicability of the synchronous-multiplex system would seem to add so greatly to its commercial value when in actual operation, that it may be well to explain it at length.

Connecting at Providence one of the sextuplex circuits established in one of the wires, to the end of the second wire at Providence, a message transmitted from Boston, over the sextuplex circuit so connected, was received in Boston on the second wire clearly and perfectly transmitted.

This experiment would seem to show that the synchronous-multiplex system is applicable not only to the connection of terminal stations, whereby the wire may be divided into the numerous circuits claimed for it, but that the six circuits, for example, obtained from a single wire, may be connected at the terminal stations at the two ends of the main line where a distributing instrument is situated, by independent wires run so as to reach the outlying cities beyond. In this manner each of these cities will be furnished with an exclusive circuit through the divided wire.

With, for example, a distributor in New York, connected by a single wire with one in Boston, and divided into say six Morse circuits, a single wire, extending to Providence, could be connected at Boston to the No. 1, of the six multiplex circuits, while Lowell, could be connected to the No. 2, of the multiplex circuits; Portsmouth to the No. 3, of the multiplex circuits; Worcester to the No. 4, of the multiplex circuits; Lawrence to the No. 5, of the multiplex circuits; and finally Lynn to the No. 6, of the multiplex circuits, thus affording each of these six cities direct circuits over one and the same wire to New York, through the medium of the distributor at Boston, without any repetition of the dispatches.

In like manner, if so desired, six cities adjacent to New York,

within distances, of say from 75 to 100 miles from New York, might be connected with Boston, through the medium of the New York distributor, or the outlying cities themselves might be put into communication with each other.

Under the present system of telegraphic communication, nearly all these outlying cities are compelled to send their messages on to Boston or New York, from which places they are repeated to their destination.

The connection above referred to may be better understood by reference to the figure. A and B represent the synchronized distributors situated at Boston and Providence respectively and connected with the main line wire Q Q. The second wire Q^1 Q^1, which ordinarily has no connection with the distributors A and B, is for the purposes of this experiment connected in the manner shown. In the figure the contacts are connected in groups of six, or in other words the main line Q Q, is divided into a sextuplex. The trailing arms at A and B, are shown in contact with one of the No. 6, contacts at d and h, respectively. Polarized relays R, R^1, R^2, R^3, and R^4, are connected respectively to five of the six circuits so provided. The sixth circuit in this case, it will be observed, is left unconnected to the polarized relay R^5. The relays are connected by means of switches S, S^1, S^2, S^3, S^4 and S^5, with the keys K, K^1, K^2, K^3, K^4 and K^5, whose front and back stops are connected with the split battery M B, grounded at Y. It will also be observed that the polarized relays R, etc., can be connected with the keys, or with the ground at X, and can therefore be used either for transmitting or receiving.

The station at Providence is similarly provided with the polarized relays R^0, R^{i}, R^{ii}, R^{iii} and R^{iv}, switches S^0, S^{i}, S^{ii}, S^{iii} and S^{iv}, keys K^0, K^{i}, K^{ii} K^{iii} and K^{iv} and main battery M^1 B^1, split and grounded at Z, and all connected as shown.

If now, the circuits being as described, the switch S, at Boston, be placed so as to connect the relay R, with the key K, a message may be sent over the main line Q, Q, to the Providence end, where it may be connected with a receiving relay and received. Instead of this, however, this circuit is in this case connected by means of the wire m, with the Providence end of the second wire Q^1, Q^1. The Boston end of Q^1, Q^1, is connected by the wire n, with the relay R^5, which we have referred to as not being connected with the remaining circuit of the sextuplex circuits. Under these circumstances the message sent from Boston to

Providence by the key K, through the relay R, is received at Boston by the receiving relay R^5, the latter relay being connected as shown by the switch S^5, to the ground at X.

When the key K, at Boston, is connected to its front stop as shown, an impulse goes out from the main battery $M B$, and traverses the following circuit, viz., through the conducting wire a, a, to key K, switch S relay R, conducting wire b, b, c, contact d, trailing arm f, conducting wire e, e, main line Q, Q, conducting wire g, trailing arm f^1 contact h, conducting wire i, j, k, l, and the remaining seven contacts to m, second main line wire Q^1, Q^1 and conductor n, from which it passes through the receiving relay R^5, where it is received, and finally to the ground at X, through o, S^5, p, q, r and s.

Now the practical value of this experiment, as has already been pointed out, consists in the very evident fact that if the message can be sent from Boston to Providence over the sextuplex circuit and received back clearly and distinctly in Boston over an independent wire, then, since it makes no difference in what direction this independent wire may extend, no matter how far its distant end may be from the synchronized distributor, within say the limit of 75 or 100 miles, important cities, lying within that distance of New York, can be readily placed in independent connection with Boston, and the outlying cities of Boston can be placed in independent connection with New York, by the operation of the two synchronized distributors A and B.

Though the leg Q^1, Q^1 in this case was but 50 miles in length, yet, from what we have already said, it is evident that much greater distances could be successfully operated in this manner. With printing instruments, since seventy-two separate circuits can be maintained, the number of cities that can be connected with one another, by means of but two synchronized distributors is clearly very great.

Central High School,
Philadelphia, July 17, 1884.

Electric Jewels.—In the ballet of the Empire theatre, at London, forty dancers have bucklers and helmets provided with incandescent electric lights covered with colored glass, so as to present the appearance of sparkling jewels. There is also a light at the point of each of the lances. The battery which furnishes the current is placed behind the shields and can work for about an hour.—*Lumière Electrique*, May 3, 1884.

AN EXTRAORDINARY EXPERIMENT IN SYNCHRONOUS-MULTIPLEX TELEGRAPHY.

By Prof. Edwin J. Houston.

A most extraordinary experiment, which is not devoid of practical bearings, has quite recently been made by Mr. Patrick B. Delany with his synchronous-multiplex telegraphic system, which is now in operation between Boston and Providence, a distance of about fifty miles.

As the experiment about to be described almost challenges belief in its possibility, I desire to state, that I have seen it myself and can vouch for the accuracy of the facts herein stated.

Wishing to try the adaptability to the synchronous system, of the automatic repeaters employed by other telegraphic systems, whereby great distances are overcome, Mr. Delany, on three different occasions during the past two weeks successfully employed such repeaters with his system, the last trial, viz., that on Monday the 14th of July, being witnessed by myself.

One of the two wires erected by the Multiplex Company between Boston and Providence, was divided into six separate and distinct Morse circuits. The first of these circuits, which we will call No. 1, was operated to Providence, at which place the receiving relay, on that circuit, was connected to the transmitting instrument on No. 2 circuit. In Boston the receiving relay, of No. 2 circuit, was connected to the transmitting instrument of No. 3 circuit. In Providence the receiving relay, of No. 3, circuit was connected to the transmitting instrument of No. 4 circuit. In Boston, the receiving instrument, of No. 4 circuit, was connected to the transmitting instrument of No. 5 circuit. Finally, in Providence the receiving relay or instrument, of No. 5 circuit, was connected to the transmitting instrument of No. 6 circuit. Under these arrangements, the transmitting instruments at both stations were operated by the receiving relays on the other circuit the same as if worked or operated by an operator; in other words the six separate and distinct circuits, established by the synchronizing apparatus between Boston and Providence, were arranged so as to form in reality a continuous wire stretched six times between Boston and Providence, with both of its free ends in Boston.

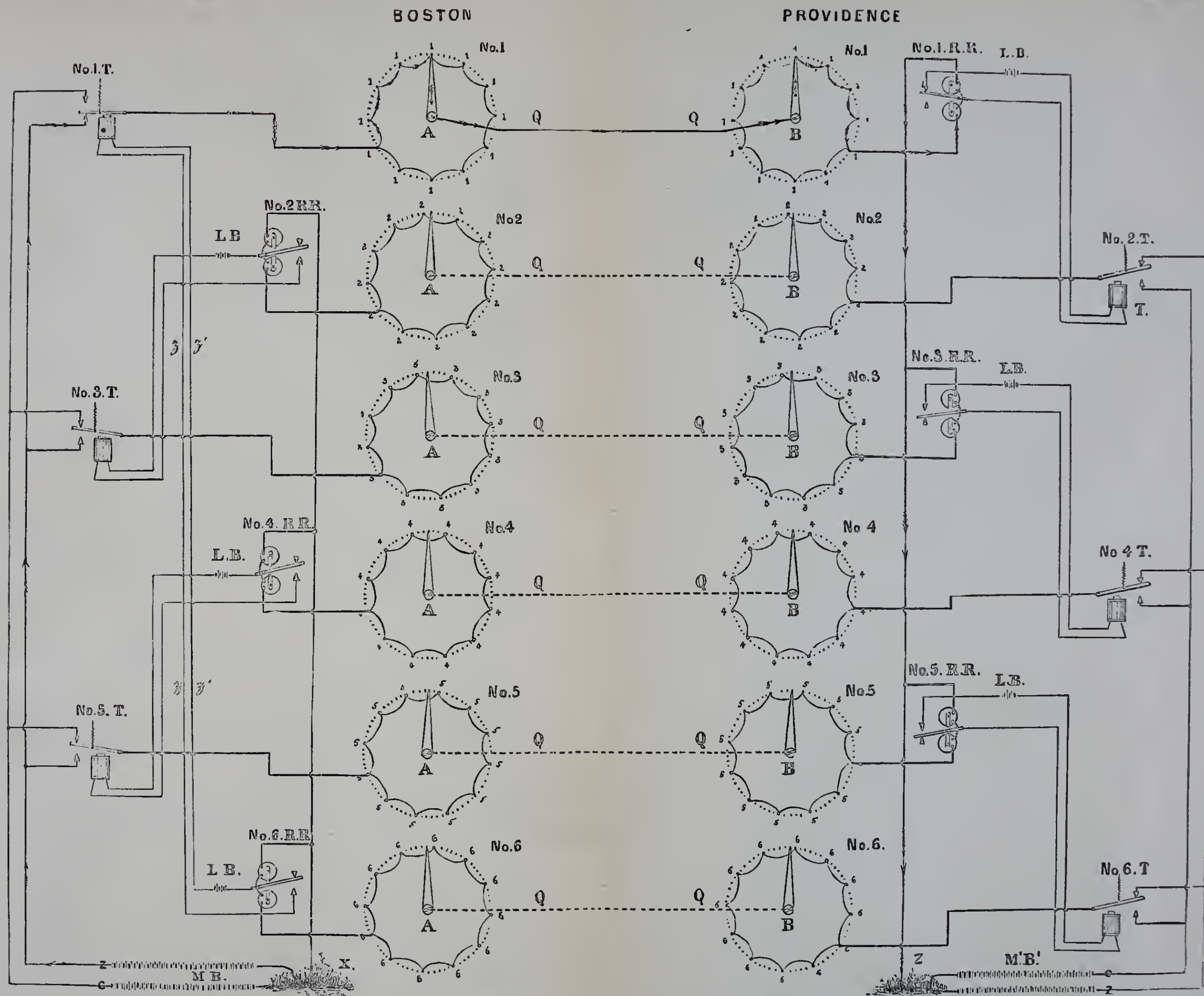
Mr. Delany then transmitted a message on the No. 1 circuit, from Boston to Providence, which was automatically retransmitted from

Providence to Boston on No. 2 circuit; again automatically retransmitted from Boston to Providence on No. 3 circuit; again automatically retransmitted from Providence to Boston on No. 4 circuit; again automatically retransmitted from Boston to Providence on No. 5 circuit, and finally automatically retransmitted from Providence to Boston on No. 6 circuit. Or, in other words, the message sent from Boston on the first circuit, went to Providence, came back to Boston, again went to Providence and came back to Boston, when it again went to Providence and came back to Boston, at which final station it was clearly read by an operator without the loss of a single character, or the slightest impairing of its original clearness, and without the aid of any person except the transmitting operator on the No. 1 circuit in Boston, and the receiving operator on the No. 6 circuit in Boston. All this was done over one and the same wire, so that the message traveled in its back and forth journeys between the two cities, about three hundred miles, or six times the distance between the two cities.

A reference to the drawing will render the preceding explanation clearer. The synchronized distributing instruments, *A* and *B*, situated at Boston and Providence, respectively, are connected by the single main line *Q Q*. The line is divided into six circuits, which we will call respectively No. 1, 2, 3, 4, 5 and 6. For the purpose of rendering the connections clearer, these six circuits have been separately represented with the sychronized distributing instruments connected therewith. It will of course be understood that but a single main line *Q Q*, furnished with but two distributing instruments, viz., one, *A*, at Boston, and the other *B*, at Providence, exists between the two cities.

This being premised, an inspection of the drawing will show that the main battery *M B*, at Boston, split and grounded at *X*, is connected with the No. 1, No. 3 and No. 5 transmitters, which are respectively connected with the No. 1, No. 3 and No. 5 sextuplex circuits of the single main line *Q Q*. At Providence the main battery, M^1 B^1, split and grounded at *Z*, is connected with the No. 2, No. 4 and No. 6 transmitters, which are respectively connected with the No. 2, No. 4 and No. 6 sextuplex circuits of the single main line *Q Q*.

At Providence, the No. 1 receiver is connected with the transmitter of No. 2 circuit, so that a message sent from Boston by No. 1 transmitter, would be received by the No. 1 receiving relay in Providence, when, by means of the local battery *L B*, would have its message



AN EXTRAORDINARY EXPERIMENT IN SYNCHRONOUS-MULTIPLEX TELEGRAPHY.

repeated by No. 2 transmitter, and sent to Boston over the No. 2 sextuplex circuit of the main line *Q Q*. This message would be received in Boston by the No. 2 receiving relay, when by means of the local battery *L B*, connected with No. 2 receiving relay, would have its message automatically repeated by the No. 3 transmitting instrument at Boston, over the No. 3 sextuplex circuit of the main-line circuit *Q Q*, to Providence, at which place it would be received by the No. 3 receiving relay. This relay, in its turn, through the aid of the local battery connected with it, automatically transmits the message through the No. 4 transmitter, over the No. 4 sextuplex circuit of the main line *Q Q*, to Boston, at which place it is received by the No. 4 receiving relay. This relay, in its turn, through the local battery *L B*, connected therewith, automatically repeats the message to the No. 5 transmitting instrument, over the No. 5 sextuplex circuit of the main line *Q Q*, to Providence, where it is received by the No. 5 receiving relay. Finally this relay, in its turn, through the intervention of the local battery *L B*, connected therewith, automatically repeats the message to No. 6 transmitter, over the No. 6 circuit to Boston, at which place it is received by the No. 6 receiving relay, by the operator stationed at the Morse instrument connected with that relay. This receiving relay is, in reality, shown in the drawing as connected with No. 1 transmitting instrument at Boston. The purposes secured by means of this connection will be hereafter explained.

Briefly, the course taken by the message in its journeys to and from the two cities, is as follows, viz.:

From Boston, by No. 1 transmitter over main line to No. 1 receiving relay at Providence.

From Providence, automatically repeated to No. 2 transmitter, and sent over main line through No. 2, sextuplex circuit to No. 2 receiving relay at Boston.

From Boston, automatically repeated to No. 3 transmitter, and sent over main line through No. 3, sextuplex circuit to No. 3 receiving relay at Providence.

From Providence, automatically repeated to No. 4 transmitter, and sent over main line through No. 4, sextuplex circuit to No. 4 receiving relay at Boston.

From Boston, automatically repeated to No. 5 transmitter, and sent over main line through No. 5, sextuplex circuit to No. 5 receiving relay at Providence.

Finally, from Providence, automatically repeated to No. 6 transmitter over the main line through No. 6, sextuplex circuit, to the No. 6 receiving instrument at Boston, where it is received by the operator.

It is not necessary, as might be supposed from the drawing, that the characters received on the group of segments comprising No. 1 circuit must, necessarily begin to return on the next adjoining segment of No. 2 circuit. Suppose, for example, that a character concluded on the segments of No. 1 circuit, where the trailing contact indicates on the drawing. By the time the armature of the relay has moved in response to this character, and has placed No. 2 circuit in connection with the battery for return transmission, the trailing contact may be on the second or third contact of the No. 2 circuit. This, however, will make no difference, since both the distributing arms are synchronous, so long as the rotating arms pass over two or three of the No. 2 segments, while the armature of the transmitting magnet is in contact with either of the poles of the battery.

When we consider that a message made up of many words, each word containing numerous letters, each letter consisting of numerous separate and distinct characters, and each character, under the synchronous multiplex system, consisting of numerous impulses, was transmitted with certainty over a single wire, back and forth, this number of times, without the slightest interruption the one with the other, the fact almost challenges belief.

While these results may appear almost incredible, what I am about to describe may at first thought seem impossible. I will endeavor, however, to give such a description of this experiment as I saw it actually made, as will persuade the reader, that so far from being impossible, its possibility must necessarily follow as a natural result of the exquisitely maintained synchronism secured by Mr. Delany's ingenious inventions.

After having successfully established by actual trial, the possibility of the use of repeaters in his synchronous system, Mr. Delany connected the relay of the sixth circuit in Boston, where the message was received, with the transmitting instrument on No. 1 circuit. Now under these conditions, on making one dot on No. 1 instrument, this dot started on its zigzag way, to and from Providence, in the manner already described, only, instead of terminating on the sixth circuit in Boston, as in the previous experiment, the same dot was automatically retransmitted into the first circuit, and again sent on

its journeying between the two cities, only on its arrival at the sixth circuit, in Boston to be again automatically retransmitted over this same winding route.

An inspection of the drawing will render this connection clearer. Instead of the message being received by an operator stationed at the No. 6, receiving relay at Boston, this instrument is furnished with a local battery, $L\ B$, and connected by means of the conducting wire $z\,z$, and $z^1\,z^1$, with the No. 1 transmitting instrument at Boston. By this means, therefore, the operator at the No. 6 receiving relay, is dispensed with, since this receiving relay again automatically sends the signal by means of the No. 1, transmitting instrument, on its ziz-zag way between the two cities, until the No. 6, transmitter at Providence, again sends it to the No. 6, receiving relay at Boston, which again automatically repeats it by the No. 1, transmitter, over the six circuits between the two cities, and so on indefinitely.

In this manner, then, the original signal kept passing from city to city, over the different circuits, in perfect rotation, without the intervention of any operator, save the one who first started the signal on its ceaseless journeyings.

Timing the intervals of the returns of the original signal between the two cities over the sextuplex circuits, it was observed that it traveled between Boston and Providence over these six circuits 300 times; or covered the distance between Boston and Providence, 1,800 times in each minute, thus making an entire distance of 1,500 miles a second, or 90,000 miles a minute; or for five minutes that a dot was kept going, the original signal, in that short time traveled no less than 450,000 miles, or eighteen times as far as the entire distance around the world at the equator.

Of course it will be understood that most of this time was taken up by the automatic movements of the armatures of the receiving relays, and the levers of the transmitting instruments. The experimental figures so obtained, however, furnish interesting data as to the rapidity, precision and certainty with which these masses of matter may be influenced by the electric current.

An observer, noticing the progress of this experiment, and reflecting on the numerous complex conditions requisite for its successful accomplishment, cannot but be singularly impressed by its extreme wierdness. Bearing in mind the exceeding complexity of structure of the synchronous-multiplex message, and the necessity for maintaining practically absolute

synchronism between the distributing and receiving instruments at each end of the main line, a feeling of incredulity almost unconsciously arises in the listener's mind. Surely this wierd traveler must miss some of his numerous connections, and once missed, his journeys are at an end forever. But when the signals are heard recurring with their automatic regularity, as though tossed to and fro between the cities by a mighty juggler; when they are heard as mysterious whisperings in the air, that follow too rapidly on one another to permit more than a part to be intelligently received, we almost lose sight of the actual conditions of the experiment, and begin to vaguely doubt whether Mr. Delany has not received a visit from Puck, who is bewildered by the rapidity with which he is forced to travel; and when the strange repetitions of the original signal follow one another with such rapidity and regularity as to produce a kind of a prolonged, but mysterious murmur we are almost disposed to believe that these sounds are the plaints of the Wandering Jew, as he ceaselessly speeds on his never-ending journey.

CENTRAL HIGH SCHOOL,
Philadelphia, August, 1884.

ON THE APPLICATION OF ELECTRICITY AS AN ILLUMINATING AGENT IN ASTRONOMICAL OBSERVATORIES.

By W. S. FRANKS.

The following notes, partly rewritten from a letter in the *English Mechanic* of April 11, 1884, and supplemented by a more detailed description of the apparatus employed, may possibly have some interest for those engaged in electrical work.

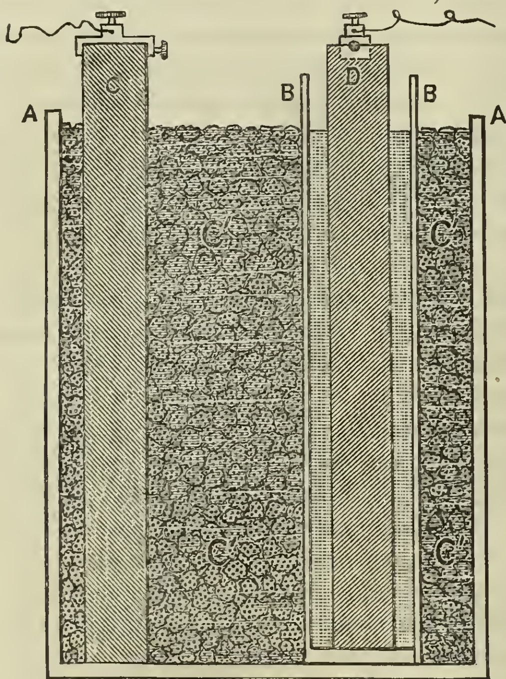
In the first place it may be premised that the efficient lighting of an observatory, including the illumination of the various instruments there used, is a matter that demands some little thought and consideration. Where a gas supply is obtainable it is a comparatively easy matter to carry pipes to the required points; but, even then, gas has many drawbacks, as well as advantages. A gas jet gives off considerable heat, is easily blown out by the wind when the observatory shutters are open, and deposits soot upon any piece of apparatus within which it may be enclosed; added to which is the difficulty of turning down the jets simultaneously, when it is necessary to use the telescopes. If gas is not procurable, and, from the circumstance that the best situation for

an observatory is in the open country, this must often be the case, resort is generally had to oils. These are still worse than gas, for the lamps become black and greasy with constant use, and are a nuisance to keep trimmed. They do, however, possess one advantage over gas, that of being readily carried about to various points. The development of the incandescent systems of Edison, Swan, and others, has given such a rapid impetus to the practicability of electric lighting, that the reason no longer exists why such manifestly imperfect illuminants should be tolerated in any well-equipped observatory, whether public or private. Especially is it adapted to the needs of that large and ever-increasing army of amateur observers, who rejoice in the possession of one of these "lighthouses of the skies." I will, as briefly as is consistent with clearness of description, give a short account of the installation of electricity as the source of light in my own observatory.

In November, 1882, a couple of $2\frac{1}{2}$-candle "Swan" lamps were fitted up in the observatory, one at the equatorial telescope, the other over a desk where the books, maps, etc., are placed. These were worked by a 4-cell bichromate battery of the ordinary kind, having one zinc and two carbon plates to each cell. The cells were of about one quart capacity each, and coupled for intensity. The plates were suspended from a wood frame, having a catgut line, working over a pulley, with a lead counterpoise; thus being easily and quickly lowered or raised. This arrangement was very convenient in use, and gave a good light at starting, but the plates became rapidly polarized, so that the light could not be depended on for more than a few minutes at a time. Another battery was added, shortly afterwards, and the two were worked alternately. In February, 1883, one of the Swan lamps fell from its support, through the spiral spring giving way, and was broken. It was replaced by another, of lower resistance, which was such an obvious improvement in light that I changed the other lamp also. Since that date, then, the same two lamps have been in use until the present time (May, 1884). The two bichromate batteries were dismounted in November, 1883, after a year's work, having, of course, been many times recharged during that interval. I then adopted a new battery, of 3 cells, on the "granule carbon" principle. The performance of this battery is so entirely satisfactory that I may be pardoned for giving a detailed description of it. Fig. 1 shows a cell in section. The outer vessel, *A*, is of salt-glazed earthenware, with a capacity of about two

gallons. *C* is the carbon plate, which is cut out of the crude material from the gas-works, merely having its edges trued with a saw. *B* is the porous cell, containing a zinc plate, *D*. The space *C′* in the outer cell is entirely filled up with pieces of broken carbon, about the size of a small nut. The charge for outer cell is one pound of bichromate of

FIG 1.

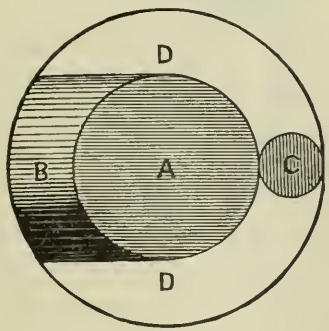


potash, dissolved in one gallon of hot water, to which is afterwards added one pound of sulphuric acid. The inner cell is charged with dilute sulphuric acid, in the proportion of 1 to 10. The three cells are coupled for intensity. When not in use, the zincs are raised out of the liquid, and suspended by loops in the insulated wire (close to binding screw) on small nails. The carbon plate was well paraffined at the exposed end, and a slip of platinum foil placed upon it, under the brass clamp; this effectually prevents oxidation of the latter by the acid creeping up the pores of the carbon. The cells will easily run for three months without recharging, their duty in observatory work being intermittent, not continuous; though, when required, the lamps can be

kept up to full incandescence for hours together. The cost of working has been, thus far, about 3*d.* per week, on the average. The light has been used, ever since its first installation (to the exclusion of any other illuminant), for reading the circles of equatorial telescope, taking time, and recording observations, as well as sketching at the telescope; all of which purposes it answers admirably. Last December (1883), I made a further adaptation of it, which is here described. In using a micrometer, transit, or any other eye-piece having wires, webs, etc., for purposes of measurement, it is necessary to slightly illuminate the field of view in order that they shall be distinctly visible on a dark sky. Under the usual conditions, this requires a somewhat complicated arrangement of lamp, prisms, counterpoise, and so forth. The particular method by which the electric light has been utilized in my own case, is, to the best of my knowledge, novel. I described it in the *English Mechanic* of April 11, 1884 (page 124), and quote therefrom, with but verbal alterations. [It is especially intended for the Newtonian reflector, which is, by far, the best known and most used construction of that class of instruments.]

"When the glasses are removed from the eye-piece, and the eye

Fig. 2.

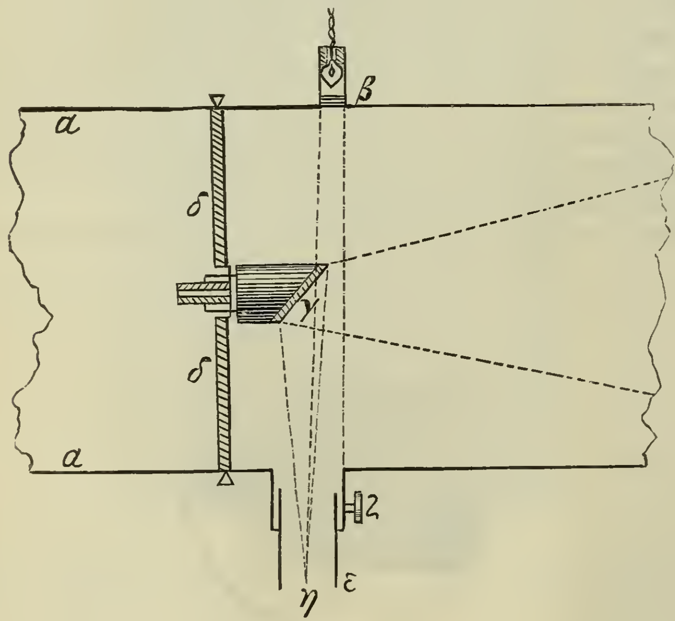


applied thereto, we see an appearance like that represented in Fig. 2, where A is the plane mirror, B part of its support, and D an annular space surrounding plane (in reality the opposite interior of main tube). On the far side of main tube a hole is pierced, so that its circular outline shall be just comprised between the outer ring bounding D and the plane A; this appears at C.

[Reference to Fig. 3 will best elucidate the general arrangement.

α is a part of the main tube, open at one end, closed by the speculum at the other; β the illuminating apparatus, placed over the hole C (in Fig. 2); γ the plane mirror, fixed at an angle of 45°; δ δ, two of the three springs carrying the plane mirror; these are seen *edgewise* from either end of the main tube; ε the draw-tube, to which the various eye-pieces are fitted; ζ the focussing screw; and η the focal point of the converging cone of rays from speculum, which impinge on the plane mirror, and thence are reflected at right angles. The direct rays from β will thus enter the eye-piece along with the reflected cone of rays from γ, as they just miss the plane mirror.]

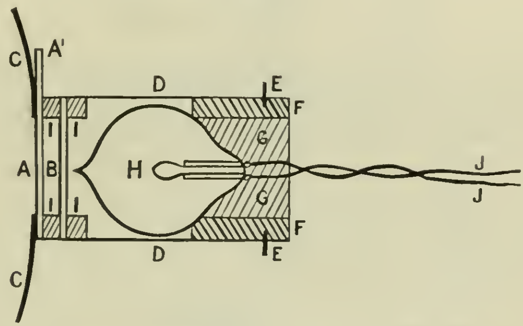
Fig. 3.



In Fig. 4 the illuminating apparatus is shown separately. A is a slip of red glass (other colors can be used at pleasure), which can readily be withdrawn or inserted by the fingers at A^1; B, a circle of opal glass, held by thick india-rubber washers I, I, on each side; this is to mitigate the light, which would otherwise be too strong; C, the base plate, of stout sheet brass, attached to main tube by four screws; D, a short piece of mandrel-drawn tubing, in which the lamp slides; E, E, pins in lamp holder, working in bayonet slots; F, a boxwood ring, accu-

rately fitting inside D; G, a cork socket for lamp; H, the "Swan" lamp ($2\frac{1}{2}$-candle); J, J, insulated wires from lamp. The tube D is soldered to base plate C, a slot being left on its upper side to receive the slip A. The boxwood ring was baked in an oven and soaked in melted solid paraffin. A good paraffined cork was tightly fitted within the boxwood ring, and hollowed at one end to receive the Swan lamp. The insulated wires were scraped, and passed through the cork, the ends being left projecting a few inches whilst being soldered to the platinum loops; on drawing back the wires the lamp was firmly pressed into its socket, and the wires twisted together nearly up to their junction with binding screws on stand of telescoe; this made a good, sound

Fig. 4.



connection. [The weak point of the ordinary Swan lamp holder is the *brass* spiral spring; this will *not* stand the damp of the observatory, as I have noticed the brass become so excessively brittle that it falls to pieces on being touched. The last time this occurred I substituted a spring of *copper* wire (this was on the desk lamp), which has shown no such tendency, and is perfectly good at the present time. However, I consider the method of mounting the lamp described above as infinitely preferable.] The resulting illumination of the field for micrometer work is, in every way, satisfactory; the lamp is instantly slid in or out of its brass socket tube, and used for reading the circles, or sketching; indeed, for the last-named purpose it is simply invaluable, the lamp being hung over the eye-piece, and a switch, within arm's length, enables the light to be instantly put on or shut off."

The electric incandescent lamp *must* be the light of the future for the observatory; it is free from the objections to gas and oils and is immensely superior to them in point of convenience.

The only observatories, that I know of, in which the incandescent lamp is used for micrometer work (the details of the way in which it is applied, seem to be different in every one), are the following:

Melbourne, Australia, R. L. J. Ellery, F.R.S.
O'Syalla, Hungary, Dr. N. de Konkoly.
Herény, Hungary, M. E. de Gothard.
Hilgay, Norfolk, England, Canon St. V. Beechey.

Observatory, 1 *High street*, *Leicester*, *England*, May, 26, 1884.

A METASTATIC HEAT REGULATOR.

By N. A. Randolph, M. D.

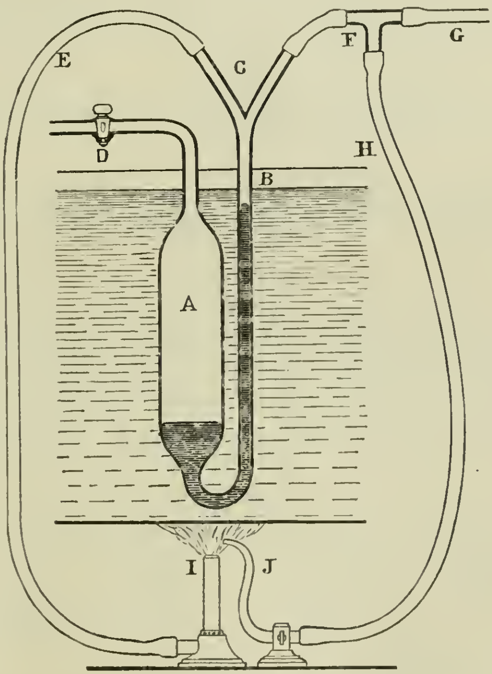
The instrument about to be described is adapted to maintain a constant temperature within any water or air chamber heated by gas, the degree of temperature thus maintained being adjustible at will.

Reference to the illustration shows an air thermometer so modified that the rise of mercury in the limb *B*, will cut off the gas supply which passes through its bifurcated extremity. A second modification lies in the accurately fitting glass stop-cock *D*, connected with the air chamber *A*. By means of this stop-cock the tension of the air within the chamber, and consequently the height of the mercury in the tube *B*, is readily adjustable. It is evident that when the mercury is forced high up in *B*, a relatively slight increase in the temperature of the surrounding medium will be sufficient to so expand the air in *A* as to force the column of mercury to the point of shut-off. On the other hand, a far higher temperature will be needed to effect the shut-off when the columns of mercury in *A* and *B* are of the same height. In practice the adjustment is effected by placing the instrument in a medium of the required temperature, the cock *D* is opened, and air slowly forced in with a syringe, until the mercurial column in *B* is nearly at the point of bifurcation; the precise height varying, of course, with the dimensions of the instrument, and being readily ascertained by practice.

The pressure of the gas employed must be kept quite low, otherwise as the mercury rises above the point of bifurcation, a portion will be blown out. One of the simpler gas pressure regulators may be advantageously inserted between the source of gas supply and the heat

regulator. It is well also that the diameter of the limb *C* should be somewhat greater than that of its fellow, and also that its point of junction with *B* should be somewhat constricted in order that a smaller variation in temperature shall effect either the patency or occlusion of the gas exit.

When the mercury rises in *B* a trifle beyond the point of bifurcation, the passage of gas from *G* to *E* is arrested, and the flame from the burner *I*, is at once extinguished. Were no further provision made,



the vessel and its contents would soon cool sufficiently to again permit the flow of gas which would then pass off, unburnt, through *I*. This difficulty is obviated by the use of a second gas jet *J*, so placed as to relight the burner *I*, upon the renewed passage of gas, and so minute as not to give out sufficient heat to counterbalance that which is lost from the vessel by radiation, etc., during the temporary stoppage in the main jet. This secondary jet may be readily made from a common brass blow-pipe, bent in the form shown in *J*, and steadily supported in such manner that its little flame may constantly play immediately

above the opening of the main burner. It is usually necessary to still further reduce the small opening of the blow-pipe by squeezing it with pliers, or by other means. The secondary flame is fed by a branch *H*, from the source of gas supply.

The instrument must be protected from touching the base of the containing vessel either by suspension or by the intervention of a plate of cork or other non-conductor. It must also be held steadily vertical, and should always be accompanied by a thermometer to verify its adjustment. It is also well to have each of the exposed surfaces of mercury covered by a drop or two of glycerine to prevent oxidation.

BIOLOGICAL LABORATORY, OF THE
UNIVERSITY OF PENNSYLVANIA, May 29, 1884.

THE DRYING OF GUNPOWDER MAGAZINES.*

By PROF. CHARLES E. MUNROE, U. S. N. A.

In the Ordnance Instructions of the United States Navy, paragraph 1233, page 341, it is directed that, in order to absorb the moisture from a magazine, chloride of *lime* or charcoal should be suspended in an open box under the arch, and that it should be renewed from time to time.

On reading this I felt assured that an error had been committed, and that it had probably arisen from the fact that the chemical names of two quite different substances, chloride of *lime* and chloride of *calcium*, are really so very much alike in sense and sound as to be very often confused, and to be even regarded as synonymous by those who are not quite conversant with them.

Chloride of *lime* is the substance which is sold in commerce under the name of bleaching powder, and it is believed to generally consist of a mixture of CaO, CaH_2O_2, $CaCl_2$ and $Ca(ClO)_2$ or $(CaO)Cl_2$. When charged as completely as possible with Cl and when in its purest form it is regarded by Kolbe† as having the composition represented by $Ca_3H_6O_6Cl_4$, which by the action of water is decomposed as follows:

$$Ca_3H_6O_6Cl_4 = CaH_2O_2 + CaCl_2 + Ca(ClO)_2 + 2H_2O.$$

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* U. S. Nav. Inst. Proc. Vol. ix.

† Ann. Ch. Phys. [4], 12, 266.

When exposed to the air the bleaching powder absorbs water, probably in proportion to the $CaCl_2$ and CaO which it contains, but it is not regarded as a deliquescent salt. At the same time it absorbs CO_2 from the atmosphere, and the calcium hypochlorite is decomposed, probably in accordance with the reaction

$$Ca(ClO)_2+H_2O+CO_2=CaCO_3+2HClO$$

or

$$Ca(ClO)_2+H_2O+CO_2=CaCO_3+H_2O+Cl_2+O.$$

Chloride of *calcium*, on the other hand, has the formula $CaCl_2$. Its most distinguishing and characteristic property is that it is highly deliquescent; that is, it possesses the power of absorbing moisture from the atmosphere, when it is exposed to it, to such a degree as to become a liquid. So deliquescent is this substance that it is always used as the example of that property when it is defined. Brandes* found that 100 parts of it, exposed to an atmosphere saturated with moisture for ninety-six days, absorbed 124 parts of water. The atmosphere has no further effect upon it than to liquefy it.

To compare the relative absorptive powers of these two substances, I exposed watch-glasses, containing, one, ordinary bleaching powder, the other chloride of calcium, side by side under a bell glass in which a vessel of water had been placed. After an exposure of three days they were weighed, and it was found that while the bleaching powder had gained 30·70 per cent. in weight, the calcium chloride had increased 60·50 per cent. The data are as follows:

	Wt. taken. Grams.	Wt. found. Grams.	Increase. Grams.	Per cent.
Calcium chloride	22·2724	36·0195	13·7471	60·50
Bleaching powder	32·9250	43·0380	10·1130	30·70

The conditions of the experiment were quite favorable to absorption of moisture by the bleaching powder, for there was necessarily but a limited supply of CO_2 in the bell glass. When it is exposed to the air the CO_2 which it absorbs forms a crust of $CaCO_3$ over its surface, which impedes the absorption of moisture.

From the consideration, then, of the hygroscopic properties of these two substances it is evident that it is the chloride of *calcium* and not the chloride of *lime* which should be used as a desiccating agent for magazines, and as it is a by-product which is obtained in enormous

* Schw. 51, 433, and Watts Dict. Chem. 1, 716.

quantities in the manufacture of soda, it ought to be obtained very cheaply. The porous chloride which has been dried at about 200° C. is better adapted for absorbing water than the fused chloride, since the latter contains both CaO and $CaCO_3$ as a result of igniting the chloride in contact with air.

In addition to the fact that bleaching powder is not the most efficient desiccating agent, either as regards its power or its price, it has occurred to me that, owing to certain other properties which it possesses, it might prove to be a very objectionable substance for use for this purpose.

It is known that after gunpowder has been stored for some time its initial velocity is reduced. This is held to be due to the absorption of moisture and the consequent efflorescence of the nitre. While recognizing the force of this explanation, I have surmised that there are other causes for this deterioration, and that one of them might be found in the slow oxidation of the sulphur, its conversion into sulphuric acid, the decomposition of the nitre with the formation of potassium sulphate and nitric acid, and then the further oxidation of sulphur by this nitric acid. The potassium sulphate thus formed would act, like the glass in Gale's process, or the graphite, charcoal, and so on, of Piobert and Fadéieff, for gunpowder; the silica in use for the silicated gun-cotton, or the camphor in the gum dynamite, to reduce the rate of inflammation, or of the transmission of the explosive undulations. The most satisfactory way for testing this theory would be by examining samples of fresh gunpowder for sulphuric acid, and then, after it had been exposed for some years to the incidents of storage and transportation which obtain in the service, to examine the same lot of powder again. I have not as yet had an opportunity for putting the theory to the test.

It, however, seemed probable to me that if oxidation, of the nature spoken of, could take place in the presence of air and moisture only, it would certainly be hastened by the presence of bleaching powder, since when the latter is exposed to the air the CO_2 absorbed decomposes it in accordance with the reactions given above by which chlorine or oxides of chlorine are liberated. These products in the presence of water are powerful oxidizing agents and will consequently act more energetically than the oxygen of the air alone. To test this I arranged an apparatus so that washed CO_2 might pass into a bottle in which bleaching powder suspended in water was placed, and the washed pro-

duct of this reaction was passed into a flask in which the gunpowder to be tested was suspended in water. The gunpowder taken for the test was Oriental.

Two portions of this powder were weighed, each being placed in a separate flask, and 200 cm. of distilled water added to it. Through one of these the gas from the bleaching powder was allowed to bubble for twelve hours and then it remained standing for some time. It was exposed to the action of the gas in all for thirty-six hours, most of the time being in strong daylight. The other flask stood, uncorked, for the same time in another room. Both were now filtered and 100 cm. of each were taken and treated with hydrochloric acid and barium chloride. The precipitate obtained in each case was washed and ignited as for the determination of sulphuric acid. The results were as follows :

	Wt. taken. grams.	Wt. $BaSO_4$ fd. grams.	Per cent. S oxidized.
Samples exposed to air,	3·4070	....	0·16
" " " bleaching powder,	4·0692	·4768	1·60

That is, that in the sample of gunpowder exposed to the bleaching powder, there was ten times as much sulphur oxidized as in that which was exposed to the air.

The method of experiment described above was employed because it was known that the state of solution would favor the change, and it was supposed that, under the conditions which prevail in magazines, a marked change would not be noticed except after a considerable length of time. However, an experiment was set on foot which imitated the conditions exactly. I put a quantity of bleaching powder in the bottom of a desiccator, and on the shelf above I put a weighed quantity of the Oriental superfine saltpetre powder, in the granulated, glazed state in which it is sold. The desiccator was then covered and set aside. At the end of twenty-six days I examined the powder, and was surprised to see an appearance of change on the surface of the powder granules ; so I immediately dissolved in hot water, filtered and precipitated with barium chloride and hydrochloric acid. For comparison, I made another determination of the sulphates in the fresh powder. The results are as follows :

	Wt. taken. grams.	Wt. $BaSO_4$ fd. grams.	Per cent. of S. oxidized.
Fresh powder,	6·6818	·0816	0·17
Powder exposed 26 days to atmosphere of bleaching powder,	6·0256	·6566	1·50

It would seem to follow, from the above results, that while the chloride of *lime* is not so efficient a desiccating agent as the chloride of *calcium*, it is at the same time very objectionable, since it *may* cause a serious deterioration of the gunpowder.

I purpose hereafter to examine samples of powder which have been acted upon by the gases from bleaching powder, by means of a method which I have recently devised for testing the incorporation of gunpowder, and I hope, before long, to have the honor of describing this method to you.

ON AN EXPLANATION OF HALL'S PHENOMENON.

By Shelford Bidwell, M.A., LL.B.

[Abstract of a paper read at the meeting of the Royal Society, Feb. 21, 1884.]

Mr. E. H. Hall's papers giving a full account of his well-known discovery are printed in the *Philosophical Magazine* for March, 1880, November, 1880, September, 1881, and May, 1883. His original experiment was as follows: A strip of gold leaf was cemented to a plate of glass and placed between the poles of an electro-magnet, the plane of the glass being perpendicular to the magnetic lines of force. The current derived from a Bunsen cell was passed longitudinally through the gold, and, before the electro-magnet was excited, two equipotential points were found by trial near opposite edges of the gold-leaf, and about midway between the ends: when these points were connected with a galvanometer there was of course no deflection. A current from a powerful battery being passed through the coils of the magnet, it was found that a galvanometer deflection occurred, indicating a difference of potential between the two points, the direction of the current across the gold leaf being opposite to that in which the gold leaf itself would have moved across the lines of force had it been free to do so. On reversing the polarity of the magnet the direction of the transverse electromotive force was reversed, and when the magnet was demagnetized the two points reverted to their original equipotential condition.

Subsequent experiments showed that the direction of the effect differed according to the metal used. Thus with silver, tin, copper, brass, platinum, nickel, aluminium and magnesium the direction of

the transverse electromotive force was found to be the same as in the case of gold: with iron, cobalt and zinc the direction was reversed, and with lead there was no sensible effect in either direction.

Hall's results may be expressed by saying that the equipotential lines across the strip are rotated in a definite direction with respect to the lines of force. This effect was attributed by him to the direct action of the magnet on the current; and very great importance has been attached to the phenomenon in consequence of the opinion expressed by Professor Rowland and others that it is connected with the magnetic rotation of the plane of polarization of light, and thus furnishes additional evidence of an intimate relation between light and electricity.

A number of experiments made by the author convinced him, however, that no direct action of the kind supposed was ever produced, and he ultimately found that Hall's phenomenon might be completely explained by the joint action of mechanical strain and certain thermo-electric effects.

The strain is produced by electro-magnetic action. It will be convenient to refer to the metallic plate or strip (which for the purposes of this explanation may be assumed to be rectangular) as if it were an ordinary map, the two shorter sides being called respectively west and east, and the two longer north and south. Let the south pole of an electro-magnet be supposed to be beneath the strip, and let the strip be traversed by a current passing through it in a direction from west to east. Then the strip will tend to move across the lines of force in the direction from south to north. Since, however, it is not free to move bodily from its position, it will be strained, and the nature of the strain will be somewhat similar to that undergone by a horizontal beam of wood which is rigidly fixed at its two ends and supports a weight at the middle. Imagine the strip to be divided into two equal parts by a straight line joining the middle points of the west and east sides. Then in the upper or northern division the middle district will be stretched and the eastern and western districts will be compressed, while in the lower division the middle part will be compressed and the two ends will be stretched. If now a current is passing through the plate from west to east, the portion of the current which traverses the northern division will cross first from a district which is compressed to one which is stretched, and then from a district which is stretched to one which is compressed; while in the southern division

the converse will be the case. And here the thermo-electric effects above referred to come into play.

Sir William Thomson, in 1856, announced the fact that a stretched copper wire is thermo-electrically positive to an unstretched wire of the same metal, while a stretched iron wire is negative to an unstretched iron wire. From this it might be inferred, as Sir William Thomson remarks, that a free copper wire is positive to a longitudinally compressed copper wire, and that a free iron wire is negative to a longitudinally compressed iron wire; and experiment shows this to be the case. *A fortiori* therefore a stretched copper wire is thermo-electrically positive to a compressed copper wire, and a stretched iron wire is negative to a compressed iron wire. If, therefore, a current is passed from a stretched portion of a wire to a compressed portion, heat will (according to the laws of the Peltier effect) be absorbed at the junction if the metal is copper, and will be developed at the junction if the metal is iron. In passing from compressed to stretched portions the converse effects will occur.

It follows from the above considerations that if the metal plate (which is subjected to a stress from south to north and is traversed by a current from west to east) be of copper, heat will be developed in the western half of the northern division and absorbed in the eastern half; while heat will be absorbed in the western half of the southern division and developed in the eastern half. But the resistance of a metal increases with its temperature. The resistance of the northwestern and southeastern districts of the plate will therefore be greater, and that of the northeastern and southwestern districts smaller than before it was subjected to the stress; and an equipotential line through the centre of the plate, which would originally have been parallel to the west and east sides, will now be inclined to them, being apparently rotated in a counter-lock-wise direction.

If the plate were of iron instead of copper the Peltier effects would clearly be reversed, and the equipotential line would be rotated in the opposite direction.

The peculiar thermo-electric effects of copper and iron discovered by Thomson are thus seen to be sufficient to account for Hall's phenomenon in the case of those metals. It became exceedingly interesting to ascertain whether the above explanation admitted of general application, and the author therefore proceeded to repeat Thomson's experiments upon all the metals mentioned by Hall. The results are

given in the following table, where those metals which in Hall's experiments behave like gold are distinguished as negative, and those which behave like iron as positive.

TABLE.

S means stretched. U means unstretched.

Metals.	Forms used.	Direction of current.	Hall's effect.
Copper	Wire and foil, pure	S. to U.	Negative
Iron	Wire and sheet, annealed	U. to S.	Positive
Brass	Wire, commercial	S. to U.	Negative
Zinc	Wire and foil	U. to S.	Positive
Nickel	Wire	S. to U.	Negative
Platinum	Wire and foil	S. to U.	Negative
Gold	Foil, purity 9·99 per cent.	S. to U.	Negative
	Wire, commercially pure	*U. to S.*	
	Jeweller's 18 ct. wire and sheet	S. to U.	
	Jeweller's 15 ct. sheet	S. to U.	
Silver	Wire and foil	S. to U.	Negative
Aluminium	*Wire and foil, pure*	*U. to S.*	*Negative?*
Cobalt	Rod, 8 mm. diameter	U. to S.	Positive
Magnesium	Ribbon	S. to U.	Negative
Tin	Foil	S. to U.	Negative
Lead	Foil (assay)	No current.	Nil

It will be seen that in every case excepting that of aluminium and one out of five specimens of gold there is perfect correspondence between the direction of the thermo-electric current and the sign of Hall's effect. With regard to the aluminium, a piece of the foil was mounted on glass and Hall's experiment performed with it. As was anticipated, the sign of the "rotational coefficient" was found to be positive, like that of iron, zinc and cobalt. Either, therefore, Mr. Hall fell into some error, or the aluminium with which he worked differed in some respect from that used by the author. The anomalous specimen of gold, being in the form of wire, could not be submitted to the same test. It probably contained some disturbing impurity.

It is submitted that the considerations and experiments above

detailed render it abundantly evident that the phenomenon described by Mr. Hall involves no new law of nature, but is merely a consequence of certain thermo-electric effects which had been observed nearly thirty years ago

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INSTRUCTION IN MECHANICAL ENGINEERING.

By Professor R. H. Thurston.

The writer has often been asked by correspondents interested in the matter of technical and trade education to outline a course of instruction in mechanical engineering, such as would represent his idea of a tolerably complete system of preparation for entrance into practice. The synopsis given at the end of this article was prepared in the spring of 1871, when the writer was on the U. S. Naval Academy, as Assistant Professor of Natural and Experimental Philosophy, and, being printed, was submitted to nearly all of the then leading mechanical engineers of the United States, for criticism, and with a request that they would suggest such alterations and improvements as might seem to them best. The results was general approval of the course, substantially as here written. This outline was soon after proposed as a basis for the course of instruction adopted at the Stevens Institute of Technology, at Hoboken, to which institution the writer was, at about that time, called. He takes pleasure in accepting a suggestion that its publication in the Journal would be of some advantage to many who are interested in the subject.

The course here sketched, as will be evident on examination, includes not only the usual preparatory studies pursued in schools of mechanical engineering, but also advanced courses, such as can be taught in special schools only, and only there when an unusual amount of time can be given to the professional branches, or when post-graduate courses can be given, supplementary to the general course. The complete course, as here planned, is not taught in any existing school, so far as the writer is aware. In his own lecture room, the principal subjects, and especially those of the first part of the work, are presented with tolerable thoroughness; but many of the less essential portions are necessarily greatly abridged. As time can be found for the extension of the course, and as students come forward better prepared for their

work, the earlier part of the subject is more and more completely developed, and the advanced portions are taken up in greater and greater detail, each year giving opportunity to advance beyond the limits set during the preceding year.

Some parts of this scheme are evidently introductory to advanced courses of study which are to be taken up by specialists, each one being adapted to the special instruction of a class of students who, while pursuing it, do not usually take up the other and parallel courses. Thus, a course of instruction in Railroad Engineering, a course in Marine Engineering, or a course of study in the engineering of textile manufactures, may be arranged to follow the general course, and the student will enter upon one or another of these advanced courses as his talents, interests, or personal inclinations may dictate. At the Stevens Institute of Technology, two such courses—Electrical and Marine Engineering—are now organized as supplementary of the general course, and are pursued by all students taking the degree of Mechanical Engineer. These courses, as there given, however, are not fairly representative of the idea of the writer, as above expressed, since the time available in general course is far too limited to permit them to be developed beyond the elements, or to be made, in the true sense of the term, advanced professional courses. Such advanced courses as the writer has proposed must be far more extended, and should occupy the whole attention of the student for the time. Such courses should be given in separate departments under the direction of a General Director of the professional courses, who should be competent to determine the extent of each, and to prevent the encroachment of the one upon another; but they should each be under the immediate change of a specialist capable of giving instruction in the branch assigned to him, in both the theoretical and purely scientific, and the practical and constructive sides of the work. Every such school should be organized in such a manner that one mind, familiar with the theory and the practice of the professional branches taught, should be charged with the duty of giving general direction to the policy of the institution and of directing the several lines of work confided to specialists in the different departments. It is only by careful and complete organization in this, as in every business, that the best work can be done at least expense in time and capital.

In this course of instruction in Mechanical Engineering, it will be observed that the writer has incorporated the scheme of a workshop

course. This is done, not at all with the idea that a school of mechanical engineering is to be regarded as a "trade school," but that every engineer should have some acquaintance with the tools and the methods of work upon which the success of his own work is so largely dependent. If the mechanical engineer can acquire such knowledge in the more complete course of instruction of the trade school, either before or after his attendance at the technical school, it will be greatly to his advantage. The technical school has, however, a distinct field; and its province is not to be confounded with that of the trade school. The former is devoted to instruction in the theory and practice of a profession which calls for service upon the men from the latter—which makes demand upon a hundred trades—in the prosecution of its designs. The latter teaches, simply, the practical methods of either of the trades subsidiary to the several branches of engineering, with only so much of science as is essential to the intelligent use of the tools and the successful application of the methods of work of the trade taught. The distinction between the two departments of education, both of which are of comparatively modern date, is not always appreciated in the United States, although always observed in those countries of Europe in which technical and trade education have been longest pursued as essential branches of popular instruction. Throughout France and Germany, every large town has its trade schools, in which the trades most generally pursued in the place are systematically taught; and every large city has its technical school, in which the several professions allied to engineering are studied, with special development of those to which the conditions prevailing at the place give most prominence and local importance.

A course of trade instruction, as the writer would organize it, would consist, first, in the teaching of the apprentice the use of the tools of his trade, the nature of its materials, and the construction and operation of the machinery employed in its prosecution. He would next be taught how to shape the simpler geometrical forms in the materials of his trade, getting out a straight prism, a cylinder, a pyramid, or a sphere, of such size and form as may be convenient; getting lines and planes at right angles, or working to miter, practicing the working of his "job" to definite size, and to the forms given by drawings, which drawings should be made by the apprentice himself. When he is able to do good work of this kind, he should attempt larger work, and the construction of parts of structures involving exact fitting and

special manipulations. The course, finally, should conclude with exercises in the construction and erection of complete structures and in the making of peculiar details, such as are regarded by the average workman as remarkable "*tours de force.*" The trade school usually gives instruction in the common school branches of education, and especially in drawing, free-hand and mechanical, carrying them as far as the successful prosecution of the trade requires. The higher mathematics, and advanced courses in physics and chemistry, always taught in schools of engineering, are not taught in the trade school, as a rule; although introduced into those larger schools of this class in which the aim is to train managers and proprietors, as well as workmen. This is done in many European schools.

As is seen above, the course of instruction in mechanical engineering includes some trade education. The engineer is dependent upon the machinist, the founder, the pattern-maker, and other workers at the trades, for the proper construction of the machinery and structures designed by him. He is himself, in so far as he is an engineer, a designer of constructions, not a constructor. He often combines, however, the functions of the engineer, the builder, the manufacturer, and the dealer, in his own person. No man can carry on, successfully, any business in which he is not at home in every detail, and in which he cannot instruct every subordinate, and cannot show every person employed by him precisely what is wanted, and how the desired result can be best attained. The engineer must, therefore, learn, as soon and as thoroughly as possible, enough of the details of every art and trade, subsidiary to his own department of engineering, to enable him to direct, with intelligence and confidence, every operation that contributes to the success of his work. The school of engineering should therefore be so organized that the young engineer may be taught the elements of every trade which is likely to find important application in his professional work. It cannot be expected that time can be given him to make himself an expert workman, or to acquire the special knowledge of details and the thousand and one useful devices which are an important part of the stock in trade of the skilled workman; but he may very quickly learn enough to facilitate his own work greatly, and to enable him to learn still more, with rapidity and ease, during his later professional life. He must also, usually, learn the essential elements and principles of each of several trades, and must study their relations to his work, and the limitations of his methods

of design and construction which they always, to a greater or less extent, cause by their own practical or economical limitations. He will find that his designs, his methods of construction, and of fitting up and erecting, must always be planned with an intelligent regard to the exigencies of the shop, as well as to the aspect of the commercial side of every operation. This extension of trade education for the engineer into several trades, instead of its restriction to a single trade, as is the case in the regular trade school, still further limits the range of his instruction in each. With unusual talent for manipulation, he may acquire considerable knowledge of all the subsidiary trades in a wonderfully short space of time, if he is carefully handled by his instructors, who must evidently be experts, each in his own trade. Even the average man who goes into such schools, following his natural bent, may do well in the shop course, under good arrangements as to time and character of instruction. If a man has not a natural inclination for the business, and a natural aptitude for it, he will make a great mistake if he goes into such a school with the hope of doing creditable work, or of later attaining any desirable position in the profession.

The course of instruction, at the Stevens Institute of Technology, includes instruction in the trades to the extent above indicated. The original plan, as given below, included such a course of trade education for the engineer; but it was not at once introduced. The funds available from an endowment fund crippled by the levying of an enormous "succession tax" by the United States government, and by the cost of needed apparatus and of unanticipated expenses in buildings and in organization, were insufficient to permit the complete organization of this department. A few tools were gathered together; but skilled mechanics could not be employed to take up the work of instruction in the several courses. Little could therefore be done for several years in this direction. In 1875 the writer organized a "Mechanical Laboratory," with the purpose of attaining several very important objects viz.: The prosecution of scientific research in the various departments of engineering work; the creation of an organization that should give students an opportunity to learn the methods of research most usefully employed in such investigations; the assistance of members of the profession, and business organizations, in the attempt to solve such questions, involving scientific research, as are continually arising in the course of business; the employment of students who

had done good work in their college course, when they so desire, in work of investigation, with a view to giving them such knowledge of this peculiar line of work as should make them capable of directing such operations elsewhere; and finally, but not least important of all, to secure, by earning money in commercial work of this kind, the funds needed to carry on those departments of the course in engineering that had been, up to that time, less thoroughly organized than seemed desirable. This "laboratory" was organized in 1875, the funds needed being obtained by drawing upon loans offered by friends of the movement and by the "Director."

It was not until the year 1878, therefore, that it became possible to attempt the organization of the shop course; and it was then only by the writer assuming personal responsibility for its expenses that the plan could be entered upon. As then organized—in the autum of 1878—a superintendent of the workshop had general direction of the trade department of the school. He was instructed to submit to the writer plans, in detail, for a regular course of shop instruction, and was given as assistant a skilled mechanic of unusual experience and ability, whose compensation was paid from the mechanical laboratory funds, and guaranteed by the writer personally, and another aid whose services were paid for partly by the Institute and partly as above. The pay of the superintendent was similarly assured. This scheme had been barely entered upon when the illness of the writer compelled him to temporarily give up his work, and the direction of the new organization fell into other hands, although the department was carried on, as above, for a year or more after this event occured.

The plan did not fall through; the course of instruction was incorporated into the college course, and its success was finally assured by the growth of the school and a corresponding growth of its income, and, especially, by the liberality of President Morton, who met expenses to the amount of many thousands of dollars by drawing upon his own bank account. The department was by him completely organized, with an energetic head, and needed support was given, in funds and by a force of skilled instructors. This school is now in successful operation. This course now also includes the systematic instruction of students in experimental work, and the objects sought by the writer in the creation of a "mechanical laboratory" are thus more fully attained than they could have possibly been otherwise. It is to be hoped that, at some future time, when the splendid bequest of Mr.

Stevens may be supplemented by gifts from other equally philanthropic and intelligent friends of technical education, among the alumni of the school and others, this germ of a trade school may be developed into a complete institution for instruction in the arts and trades of engineering; and may thus be rendered vastly more useful by meeting the great want, in this locality, of a real trade school, as well as fill the requirements of the establishment of which it forms a part, by giving such trade education as the engineer needs, and can get time to acquire.

The establishment of advanced courses of special instruction in the principal branches of mechanical engineering may, if properly "dovetailed" into the organization, be made a means of somewhat relieving the pressure that must be expected to be felt in the attempt to carry out such a course as is outlined below. The post-graduate or other special departments of instruction, in which, for example, railroad engineering, marine engineering, and the engineering of cotton, woolen, or silk manufactures, are to be taught, may be so organized that some of the lectures of the general course may be transferred to them, and the instructors in the latter course thus relieved, while the subject so taught, being treated by specialists, may be developed more efficiently and more economically.*

Outlines of these advanced courses, as well as of the courses in trade instruction comprehended in the full scheme of mechanical engineering courses laid out by the writer a dozen years ago, and as since recast, might be here given, but their presentation would occupy too much space, and they are for the present omitted.

The course of instruction in this branch of engineering, at the Stevens Institute of Technology, is supplemented by "Inspection Tours," which are undertaken by the graduating class toward the close of the last year, under the guidance of their instructors, in which expeditions they make the round of the leading shops in the country, within a radius of several hundred miles, often, and thus get an idea of what is meant by real business, and obtain some notion of the extent of the field of work into which they are about to enter, as well as of the importance of that work and the standing of their profession among the others of the learned professions with which that of engineering has now come to be classed.

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* The workshop course may be similarly relieved by the preparatory training of younger boys, who may be taught the use of tools before entering the higher schools.

At the close of the course of instruction, as originally proposed, and as now carried out, the student prepares a "graduating thesis," in which he is expected to show good evidence that he has profited well by the opportunities which have been given him to secure a good professional education. These theses are papers of, usually, considerable extent, and are written upon subjects chosen by the student himself, either with or without consultation with the instructor. The most valuable of these productions are those which present the results of original investigations of problems arising in practice or scientific research in lines bearing upon the work of the engineer. In many cases, the work thus done has been found to be of very great value, supplying information greatly needed in certain departments, and which had previously been entirely wanting, or only partially and unsatisfactorily given by authorities. Other theses of great value present a systematic outline of existing knowledge of some subject which had never before been brought into useful form, or made in any way accessible to the practitioner. In nearly all cases, the student is led to make the investigation by the bent of his own mind, or by the desire to do work that may be of service to him in the practice of his profession.* All theses are expected to be made complete and satisfactory to the head of department of engineering before his signature is appended to the diploma which is finally issued to the graduating student. These preliminaries being completed, and the examinations having been reported as in all respects satisfactory, the degree of Mechanical Engineer is conferred upon the aspirant, and he is thus formally inducted into the ranks of the profession.

COURSE OF INSTRUCTION IN MECHANICAL ENGINEERING.

Robert H. Thurston—July, 1871.

I.

MATERIALS USED IN ENGINEERING.—Classification, Origin, and Preparation (where not given in course of Technical Chemistry), Uses, Cost.

Strength and Elasticity.—Theory (with experimental illustrations), reviewed, and tensile, transverse and torsional resistance determined.

Forms of greatest strength determined. *Testing* materials.

Applications.—Foundations, Framing in wood and metal.

FRICTION.—Discussion from Rational Mechanics, reviewed and extended.

Lubricants treated with materials above.

Experimental determination of "coefficients of friction."

* Some of these papers have been published in the JOURNAL OF THE FRANKLIN INSTITUTE, and other periodicals, as valuable contributions to technical literature.

II.

TOOLS.—Forms for working wood and metals. Principles involved in their use.

Principles of pattern making, moulding, smith and machinists' work so far as they modify design.

Exercises in Workshop in mechanical manipulation.

Estimates of cost (stock and labor).

MACHINERY AND MILL WORK.—Theory of machines. Construction. Kinematics applied. Stresses, calculated and traced. Work of machines. Selection of materials for the several parts. Determination of *proportions* of details, and of *forms* as modified by difficulties of construction.

Regulators, Dynamometers, Pneumatic and Hydraulic machinery. Determining *moduli* of machines.

POWER, transmission by gearing, belting, water, compressed air, etc.

LOADS, transportation.

III.

HISTORY AND PRESENT FORMS OF THE PRIME MOVERS.

Windmills, their theory, construction, and application.

Water Wheels. Theory, construction, application, testing, and comparison of principal types.

Air, Gas, and Electric Engines, similarly treated.

STEAM ENGINES.—Classification. [Marine (merchant) Engine assumed as representative type.] Theory. Construction, including general design, form and proportion of details.

Boilers similarly considered. Estimates of cost.

Comparison of principal types of Engines and Boilers.

Management and repairing. Testing and recording performance.

IV.

MOTORS APPLIED TO MILLS. Estimation of required power and of cost.

Railroads. Study of Railroad machinery.

Ships. Structure of Iron Ships and Rudiments of Naval Architecture and Ship Propulsion.

PLANNING Machine shops, Boiler shops, Foundries, and manufactories of textile fabrics. Estimating cost.

LECTURES BY EXPERTS.

GENERAL SUMMARY of principal facts, and natural laws, upon the thorough knowledge of which successsful practice is based; and general *resumé* of principles of business which must be familiar to the practicing engineer.

V.

GRADUATING THESES.

GRADUATION.

Accompanying the above, are courses of instruction in higher mathematics, graphics, physics, chemistry, and the modern languages and literatures.

REPORT ON THE TRIAL OF THE "CITY OF FALL RIVER."

By J. E. SAGUE, M. E., and J. B. ADGER, M. E., with an introduction by Professor R. H. THURSTON.

(Concluded from vol. cxviii., page 115.)

COAL CONSUMPTION.

As above shown it was impossible to make an exact boiler test, and it was therefore impossible to get an exact figure for the number of pounds of coal per horse power per hour used by the engine.

It was at first supposed by the authors that it would be perfectly fair, for the engine, to assume that the fire was the same at Throggs Neck, as it was on leaving Fall River, *i.e.*, that there was the same depth of coal on the grate and the same porportion of ashes in this depth. It was thought that this assumption would give the closest appoximation possible and a very nearly correct figure. Upon examination however it was found that a difference of four inches in the depth would make a difference in the weight of coal per horse power per hour of about $\frac{1}{4}$ pound. When one considers the heat and glare to which the eye was exposed on opening the furnace door, the fire being nearly white hot, the probability of one's being able to judge within three or four inches of the comparative depths of the two fires becomes very small. Then, too, the assumption that the proportion of ash on the grate at the start, from the bank, and the 3,000 or 4,000 pounds of coal thrown on within two hours of starting, is the same as after the fire has been burning fiercely for nine or ten hours, even though the fire has been sliced once or twice, admits of too great an error to carry conviction. The above assumption therefore could not be supported and that method of obtaining a figure for the coal consumption of the engine had to be abandoned.

The most closely approximate figure must be obtained from the boiler test, as follows. The average actual evaporation of the two boilers is divided into the number of pounds of water used by the engine per horse power per hour. For example, on the night of May 10th, the number of pounds of water evaporated from the temperature of the feed in the forward boiler was 7·939 pounds, and in the after boiler 8·156 pounds, average 8·047 pounds. The water per horse-

power per hour, obtained from the meters, was 17·173 pounds; dividing this by 8·047, we obtain 2·13 pounds for the coal per horse-power per hour. This of course may contain a slight error due to the assumptions made as above; but, we think that it is an outside figure, and consequently a safe one, and may be taken as practically correct.

The same figure for the other nights will be found in the table below. The amount of coal used per day on the regular trips, which may be called the commercial coal figure, was obtained very exactly, and this is the figure which specially interests the owners. For the sake of having a figure convenient in comparisons, we shall divide by the number of hours the engine was actually running on each day and also by the number of horse-power developed, getting thus the number of pounds of coal per horse-power per hour.

This commercial coal figure was gotten both when the compound engine was running and also after the high pressure cylinder was thrown off and the engine was run as a simple engine with only the large cylinder of the engine connected. In the latter case however the commercial coal figure is not strictly exact, but is closely approxinate. This commercial figure includes all the coal used per day; that used in the banks and getting up steam as well as that actually evaporating the water while the boat is under way.

	B	C	D	E
Date of experiment	May 4	May 9	May 10	June 7
Average temperature of chimney gases	460°	440°	455°	612°
No. of pounds of coal used by engine, per indicated horse-power per hour	2·045	2·09	2·13	
No. of pounds of combustible used by engine per indicated horse-power per hour	1·68	1·737	1·755	
Total No. of pounds of coal used by engine per indicated horse-power per hour	2·21	2·28	2·34	3·477

From the figures in the above table it is seen that the *City of Fall River* running with her compound engine, burns about 2·3 pounds of coal per horse-power per hour; while, when run, without her high pressure cylinder, as a simple engine, she requires 3·477 pounds of coal. The difference is 1·177 pounds per horse-power per hour, or running 11 hours a day and developing 1·600 horse-power the differ-

ence is very nearly 10 tons of coal a day, or 20 tons a round trip. That this difference cannot be attributed wholly to the engine is shown by the fact that the temperature in the chimney is much higher when the simple engine is used, and that therefore the efficiency of the boilers is materially decreased on account of their being very much forced to furnish the steam required by the simple engine as it was run upon June 7th.

RESULTS OF ENGINE TESTS.

The appended table contains the data and results of the tests made upon the engines. Most of the figures are sufficiciently explained in the table and only such as are the result of computation will be discussed here.

The columns designated with the first five letters of the Alphabet. A, B, C and D are the tests of the compound engine, and E was made after the high pressure cylinder had been disconnected and while the engine was running as a simple engine. The date of each test is prefixed to its appropriate column. The various pressures, temperatures, etc., are the average figures for the pressures, temperatures, etc., during the test and are obtained from numerous observations made at regular intervals. The fraction of stroke completed when the steam is cut off is found for each cylinder by taking on the cards the point of cut-off, and finding the average distance from this point back to the commencement of stroke. This distance is then divided by the average length of the cards and the quotient is the "fraction" sought. This is done for both the effective point of cut-off, which is the point of intersection of the admission line and the expansion line both prolonged, and is the point at which cut-off would take place if there were no wire-drawing, and also for the point of virtual cut-off, which is the point on the cards at which admission closes.

The number of revolutions per minute is the average figure, taken from the cards, upon each of which was carefully noted the number of revolutions the engine was making at the time the card was taken.

The indicated pressure on the piston at the various parts of the stroke were found from the indicator cards. The back pressure against the piston at the commencement of stroke is the pressure in the clearance spaces; and this is found, as the others are, from the cards, the point at which compression ceases being noted on them.

The mean effective pressure is found from the cards by dividing

the mean area, found with a carefully tested planimeter, by the mean length, and multiplying the quotient by the scale of the spring used in the indicator.

The mean back pressure during the stroke is found from the cards, by dividing the mean area between the back pressure line and the line of perfect vacuum by the mean length of the cards.

The horse-power is found for the top and bottom of each cylinder separately, and the area of the piston rod is deducted from the area acted upon by the steam in the case of the top of each cylinder.

The number of pounds of feed water consumed per horse-power per hour is found by the meters, the dial readings giving the number of cubic feet passed into the boiler during the test. From the meter tests, described above, the exact weight of each cubic foot was found; and the total number of pounds divided by the length of test in hours and by the horse-power developed by the engine gives the figures entered in the table.

The number of Fahr. heat-units consumed per hour per horse-power developed by the engine is found by getting the number of heat units absorbed by each pound of water, *i.e.*, by subtracting the temperature of the feed water from the total number of thermal units contained in one pound of steam at boiler pressure, and multiplying this difference by the number of pounds of water consumed per hour per indicated horse-power.

The number of times the steam was expanded is found for the compound engine by dividing the volume of the low pressure cylinder, plus the clearance spaces at one end, by the volume of the high pressure cylinder up to the point of effective cut-off, plus the clearance spaces of the high pressure cylinder at one end.

For the simple engine the volume of the cylinder, plus the clearance space of one end, is divided by the volume of the cylinder up to the point of cut-off, plus the clearance space at one end.

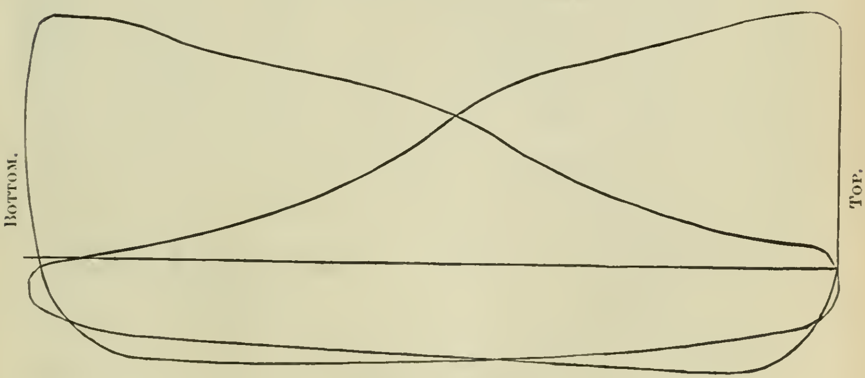
The number of pounds of steam accounted for by the indicator per hour per horse-power, at any point in the stroke, is found as follows:

The volume displaced by the piston up to the given point is added to the volume in clearance and steam passage at one end, and the sum multiplied by the weight of the unit of volume of steam at the pressure corresponding to the given point. From this product is deducted the weight of steam already present in the clearance space at one end at the commencement of stroke, using the weight of a unit of volume

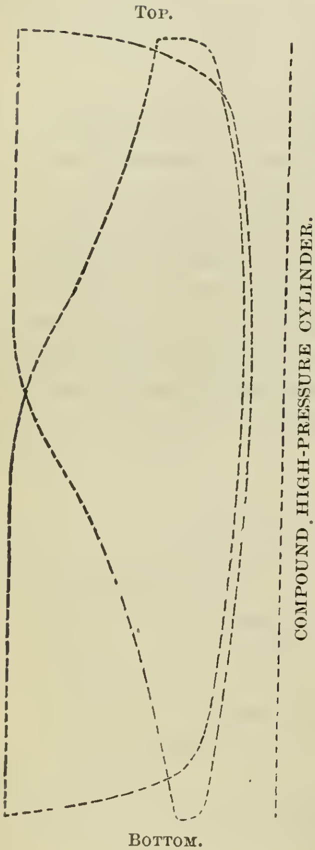
of steam of the pressure at the end of compression or just at the beginning of the return stroke. This result is multiplied by the number of single strokes per hour and is the weight of steam accounted for by the indicator for that period. This we have divided by the horse-power for convenience in comparison.

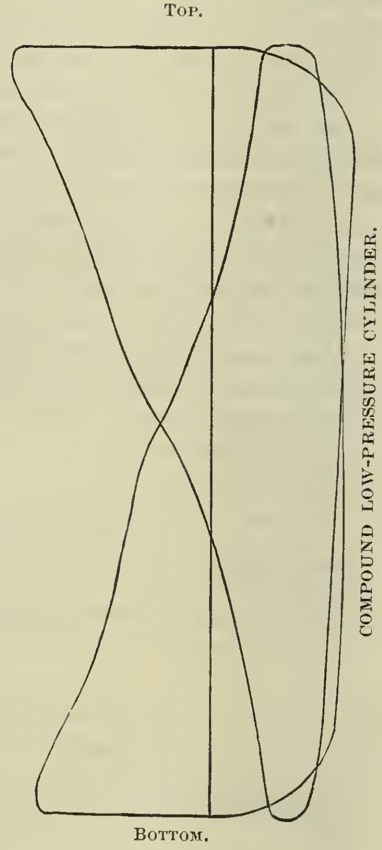
The number of pounds of condensing water per horse-power is obtained as follows: $33{,}000 \times 60 \div 772$ gives the number of thermal units converted into one horse-power per hour. This figure is subtracted from the number of thermal units consumed by the engine per horse-power per hour and the difference is the number of heat units that goes into the condenser per horse-power per hour. This is then divided by the difference between the temperatures of the condensing water at its entrance to, and exit from, the condenser and the quotient is the number of pounds of condensing water passing through the condenser per horse-power per hour, the losses by conduction and radiation being neglected.

FIG. 3.—INDICATOR DIAGRAMS.



SIMPLE ENGINE.


TOP.
BOTTOM.
COMPOUND, HIGH-PRESSURE CYLINDER.


TOP.
BOTTOM.
COMPOUND LOW-PRESSURE CYLINDER.

RESULTS OF TRIALS OF THE "CITY OF FALL RIVER."

	A	B	C	D	E
Date of Test.	May 3	May 4	May 9	May 10	June 7
Mean steam pressure in boiler per guage	68·5	69	70	70	28·5
Mean steam pressure in receiver per guage	10	10·5	11	11	
Position of throttle valve	Open wide	Open wide	Open wide	Open wide	Open 3/8ths
Fraction of stroke in small cylinder completed before point of effective cut-off	....	....	....	·445	
Fraction of stroke in small cylinder completed at point of virtual cut-off	....	....	....	·548	
Fraction of stroke in large cylinder completed at point of virtual cut-off	....	....	....	·488	·445
Number of times steam was expanded	....	....	....	6·989	2·168
Height of barometer in inches of mercury	30·54	30·7	30·7	30·5	30·37
Vacuum in condenser in inches of mercury	28	28	28·6	28·4	27
Number of double strokes made per minute by pistons	28·53	25·91	25·53	25·71	23,87
Number of pounds of feed water pumped into boilers per hour.	26696	27718	27834	27854	35271
Temperature in Fahr. degrees of feed water	104·2°	101·65°	97°	97°	111°
Temperature in Fahr. degrees of atmosphere	82°	74·6°	78°	77·5°	82·4°
Temperature in Fahr. degrees of sea water	47·25°	47·64°	49°	49·38	59·22°
Temperature in Fahr. degrees of water at exit from condenser	95·57°	92·48°	89°	90°	104
Speed of vessel per hour in statute miles	17·5	15·94	16	17·3	15
Slip of centre pressure of wheels in per centum of speed according to Rankine's rules	10·2	....	20·4	....	
Mean draft of vessel	10′ 7″	10′ 5″	10′ 7″	10′ 65″	10′ 6″
Displacement at draft (gross tons)	1948	1908	1948	1938	1928
Indicated pressures on piston of small cylinder in lbs. per sq. in. above zero.					
Indicated at commencement of stroke	....	....	....	82	
Indicated at point of virtual cut-off	....	....	....	74·3	

RESULTS OF TRIALS OF THE "CITY OF FALL RIVER."—*Continued.*

	A	B	C	D	E
Date of test.	May 3	May 4	May 9	May 10	June 7
Indicated at end of stroke	....	....	....	42·675	
Back pressure against piston of small cylinder at commencement of stroke in lbs. per sq. in. above zero	....	....	....	75·125	
Indicated pressure on piston of small cylinder in lbs. per sq. in. (mean effective)	40·7	841·16	41·28	41·76	
Indicated pressures on piston of large cylinder in lbs. per sq. in. above zero	....	....	....	....	
Indicated at point of virtual cut-off	....	....	....	17·416	28·66
Indicated at end of stroke	....	....	....	9·47	14·74
Mean back pressure against the piston of large cylinder during stroke in lbs. per sq. in above zero	....	....	....	4·89	
Back pressure against piston of large cylinder at commencement of stroke in lbs. per sq. in. above zero	....	....	....	12·904	17·3
Indicated pressure on piston of large cylinder in lbs. per sq. in. (mean effective)	12·24	12·309	12·56	12·43	21·6
Indicated horse-power developed in small cylinder (top)	361·84	370·51	362·45	369·735	
Indicated horse-power developed in small cylinder (bottom)	396·52	407·45	406·33	413·18	
Indicated horse-power developed in large cylinder (top)	423·04	340·09	433·46	432·046	652·13
Indicated horse-power developed in large cylinder (bottom)	396·90	407·28	408·9	407·061	694·85
Aggregate indicated horse-power developed by engine	1578	1615	1611	1622	1347
Number of lbs. of feed water consumed per hour per indicated horse-power	16·972	17·157	17.276	17·173	26·185
Number of Fahr. heat units consumed per hour per indicated horse-power	18690	19005	19211	19090	28405
Weight of steam in lbs. per hour in small cylinder.					
Weight calculated from pressure at point of virtual cut-off	....	....	....	25076	
Weight calculated from pressure at end of stroke	....	....	....	25676	
Weight of steam in lbs. per hour in large cylinder.					
Weight calculated from pressure at point of virtual cut-off	....	....	....	20697	31224·75
Weight calculated from pressure at end of stroke	....	....	....	22421	31798·74

RESULTS OF TRIALS OF THE "CITY OF FALL RIVER."—*Continued.*

	A	B	C	D	E
Date of test.	May 3	May 4	May 9	May 10	June 7
Weight of steam in lbs. per indicated horse-power per hour.					
Weight in small cylinder at point of cut-off (virtual)	....	....	....	15·46	
Weight in small cylinder at point of release	....	....	....	15·83	
Weight of steam in lbs. per indicated horse-power per hour.					
Weight in large cylinder at point of virtual cut-off	....	....	....	12·76	23·18
Weight in large cylinder at point of release	....	....	....	13·82	23·607
Number of lbs. of water pumped through the condenser per indicated horse-power per hour	333·8	366·4	416·156	406·8	577·04

THERMODYNAMIC EFFICIENCY OF ENGINE CONSIDERED AS PERFECT.

The thermodynamic efficiency of the steam as used in the compound engine on May 10th was computed according to the method given by Rankine (Steam Engine, Art. 284). The pressures of admission and of release and the mean back pressure were obtained from the cards and the corresponding temperatures, densities and latent heats arrived at by using the formulas given in the "Steam Engine" for them (Arts. 206, III, and 255).

The following are the data and results:

p_1 = absolute pressure of admission = 11808 lbs. per square foot.
p_2 = absolute pressure of release = 1363·68 lbs. per square foot.
p_3 = mean absolute back pressure = 704·16 lbs. per square foot.
t_4 = absolute temperature of feed water = 558·36° Fahr.

The corresponding temperatures, densities and latent heats are designated by the same subscripts.

$t_1 = 774{\cdot}50°$ Fahr. $\qquad t_2 = 652{\cdot}32°$.
$L_1 = 131841{\cdot}14$. $\qquad L_2 = 19000{\cdot}39$.
$D_1 = {\cdot}1909$. $\qquad D_2 = {\cdot}02606$.

From these data the following results were arrived at, by considering the cylinders as non-conducting and the engine perfect. The Rankine formulæ on pages 388 and 389 were employed.

The ratio of expansion $r = 6{\cdot}7167$.
Energy per cubic foot of steam admitted $UD_1 = 27183{\cdot}43$ foot-lbs.
Heat expended per cubic foot of steam admitted $H_1D_1 = 163716{\cdot}507$ foot-lbs.
Mean effective pressure, or energy per cubic foot swept through by piston.

$$\frac{UD_1}{r} = 4047{\cdot}5 \text{ lbs. per square foot.}$$

Heat expended per cubic foot swept through by the piston,

$\dfrac{H_1D_1}{r} = 24{,}377$ lbs. on square foot = pressure equivalent to heat expended.

Efficiency of steam $= \dfrac{UD_1}{H_1D_1} = \dfrac{U}{H_1} = {\cdot}166$.

Net feed water per cubic foot swept through by piston $= \dfrac{D_1}{r} = {\cdot}0284$.

Cubic feet to be swept through by piston for each indicated horse-power per hour $= \dfrac{1{,}980{,}000}{M.E.P_1 = 4047{\cdot}5} = 489{\cdot}2$ cubic feet.

Feed water per indicated HP per hour $= 489{\cdot}2 \times {\cdot}0284 = 13{\cdot}89$ lbs.
Actual feed water, $= 17{\cdot}00$ lbs., nearly.

Difference, $3{\cdot}11$ lbs. $= 22$ p. c.
due to cylinder condensation other than that required to perform the work, and other wastes not taken into account in the theory.

CONCLUDING REMARKS.

In these experiments, the data have been obtained and utilized as far as possible with a view to tracing the development and consumption of the energy stored in the coal; and while our results prove the economy to be as great, probably, as that of any previous combination of marine machinery, they also call attention to the great disproportion between the total, and effectively utilized, power, which modern engineering has as yet found little means of reducing.

The results obtained during these experiments show strikingly the relative economy of a somewhat high ratio of expansion and change of type, compared with that ordinarily used upon steamers of this class. The experiments do not in themselves give any marked result as to the relative advantages of simple and compound engines; but as these cylinders were unjacketed, and merely dry steam was used, the objection cannot be urged that compounding has here been united with the improvements and advantages of superheated steam, high pressures, or steam jacketing, and single cylinder engines having about the same ratio of expansion, may furnish valuable examples for comparison.

In the case in hand, however, it would be impracticable to use a ratio of expansion of nearly seven with a single cylinder owing to the great changes of pressure and consequent loss of smoothness in running, as well as other disadvantages of great fluctuations of this sort. The difference of coal consumption is evidently owing in a large degree to the variation of boiler efficiency, the temperature of chimneys in the simple test being much in excess of that in the compound, and consequently the waste of heat greater.

The questions arising, relating to the comparative cost of the two engines, the space they occupy, cost of repairs, etc., all of which enter into the relative commercial efficiency require further data and to some extent can only be settled by experience. These points, however, together with the comparative value of feathering and radial wheels, jet and surface condensers, the most economical point of cut-off, steam jacketing, and other unsettled points in steam engineering upon which these experiments may have a bearing, open a field quite beyond the limits of the present paper.

The authors, in conclusion, would acknowledge their obligations to Mr. Stevenson Taylor, and to the other members of the W. & A. Fletcher Co. for unceasing kindness and attentions throughout the trials, and for securing to them the opportunities for making a thorough test. To Mr. Root, of the Worthington Hydraulic Works, for valued aid and counsel, and to the Fall River Line and its employees for cheerfully rendered voluntary assistance. For aid in the experimental portion of their work, the authors are indebted to Messrs. Barnes, W. Carroll, Maury and Whiting, of the Stevens Institute of Technology, and to Mr. Andrew Fletcher, Jr., of New York.

APPENDIX.

SUMMARY:—The following statement, including other and later figures than those given in the preceding paper, has been prepared, and is kindly supplied by the W. & A. Fletcher Co.

STEAMER "CITY OF FALL RIVER."

Side-wheel Freight Boat of The Old Colony Steam Boat Co., plying between New York, Newport, R. I., and Fall River, Mass. Compound vertical beam engine, H. P., cylinder, $44'' \times 8'$; L. P. cylinder, $68'' \times 12'$. Built by W. & A. Fletcher, North River Iron Works. New York, 1883, and so constructed that the high pressure cylinder can be entirely disconnected, leaving a simple beam engine, cylinder, $68'' \times 12'$.

MEMORANDUM OF RESULTS OF EXPERIMENTS

made to show the relative economy in fuel between a Compound Engine and a Simple Engine.

Trip No.	Date.	Style of engine.	Ports.	Distance statute miles.	Draft of water.	Displacement, gross tons.	Running time.	Average pressure steam per guage.	Speed per hour.	Revolutions.	Revolutions per minute.
	1883.				ft. in.		h. m.		Stat. miles		
1	May 3.....	Compound ...	New York to Fall River.	179	10 7	1948	10 33	68½	16·96	15833	25·01
2	May 4.....	Compound ...	Fall River to New York.	179	10 5	1908	11 35	69	15·45	17711	25·5
3	May 9.....	Compound ...	New York to Fall River.	179	10 7	1948	11 18	70	15·84	17392	25·65
4	May 10...	Compound ...	Fall River to New York.	179	10 6½	1938	10 46	70	16·62	16669	25·8
5	June 7....	Simple..........	Fall River to New York.	179	10 6	1928	12 04	28	14·83	17286	23·87
6	June 11...	Simple..........	New York to Newport...	160	10 7	1948	10 26	66	15·34	15287	24·42
7	June 12...	Simple..........	Newport to New York.	160	10 6	1928	10 40	25½	15·	15415	24·1

Trip No.	Date.	Style of engine.	Ports.	Horse-power.	Coal in pounds.	Coal per hour, pounds.	Coal per hour per horse-power.	Water per hour in pounds.	Feed temp'rature of water.	Water per hour per horse-power.	Tide, etc.
	1883.						lbs.			lbs.	
1	May 3.....	Compound	New York to Fall River.	1578	......	......	......	26696	104·2	17·	fair.
2	May 4....	Compound	Fall River to New York.	1615	.....	3300	2·04	27718	101·6	17·16	ahead.
3	May 9....	Compound	New York to Fall River.	1611	36800	3257	2·0217	27834	97·	17·3	ahead.
4	May 10...	Compound	Fall River to New York.	1622	35609	3307	2·0388	27854	97·	17·22	fair.
5	June 7....	Simple.......	Fall River to New York.	1347	......	......	......	35271	111·	26·2	ahead.
6	June 11...	Simple.......	New York to Newport...	1472	43000	4121	2·8	......	......	......	even.
7	June 12...	Simple.......	Newport to New York.	1457	44150	4139	2·84	......	......	......	fair.

GENERAL NOTES.

Fires are well burnt down at end of each trip. When boat arrives at dock, fires are banked and coal put on to keep them alive while steam is blown off. During the day, about noon, more coal is put on fires. An hour previous to departure fires are hauled and spread again with fresh coal, to make steam, and ordinarily no further firing is necessary for half an hour after starting.

On each trip indicator cards were taken every half hour. Water was measured by Worthington's meters, readings taken every hour; meters tested by measuring water, under the same pressure as in pipes when feeding boilers, into a barrel, and weighing four cubic feet at a time; variations of such tests being from 61·4 to 61·5 lbs. per cubic feet. On June 10, the cut-off on Simple engine was shortened, making it easier to keep steam and to run with wide throttle. If meters had not been removed before this the water test would have shown better results per horse-power per hour than was shown on trip No. 5. All Wilkesbarre coal, mined by Lehigh and Wilkesbarre Coal Co. All water measurements, power, calculations and coal measurements of trips Nos. 1, 2, 3, 4, 5 were made by Messrs. Adger and Sague, of the Stevens Institute of Technology, Hoboken, N. J. The power and coal measurements of trips Nos. 6 and 7 were made by W. & A. Fletcher Company.

SPECIAL NOTES.

Trip No. 1, no coal measured. Trip No. 2, coal measured for ten hours run, and of 77½ boxes used 12 were weighed, and average coal per hour found to be 3,300 lbs. Trip No. 3, 6,062 lbs. coal put on fires in New York before starting, to spread fires and make steam. 32,800 lbs. coal put on fires between New York and Fall River. Assuming that it required all of this latter amount, and, in addition, 4,000 lbs. of that put on before starting, makes total consumption of coal 36,800 lbs., as per table. Trip No. 4, 6,711 lbs. coal put on to cover fires and make steam, and 31,609 lbs. on trip. With same assumption as in trip No. 3, makes total consumption 35,609 lbs., as per table. Trip No. 5, no coal weighed. Trip No. 6, 39,000 lbs. coal used on trip. With same assumption as in trip No. 3 and 4, makes total consumption 43,000 lbs., as per table. Trip No. 7, 40,150 lbs. coal used on trip. With same assumption as in trip No. 3 and 4, makes total consumption 44,150 lbs., as per table.

Memorandum of coal as weighed for round trip, May 9 *and* 10.

	Lbs.
May 9, in New York. To make steam before leaving, 6,062 lbs., on trip to Fall River, 32,800 lbs.	= 38,862
May 10, in Fall River. On banks morn and noon, 2706 lbs., to make steam before leaving, 6,711 lbs.	= 9,417
On trip to New York, 31,609 lbs.; May 11, on banks morn and noon, 2,598 lbs.	= 34,207
Coal used in main boilers	= 82,486
Coal used in donkey boiler and kitchen	= 2,634
Total coal charged to boat	= 85,120
Deduct coal used by engine, as per table	= 72,409
Leaves nearly 3 tons per single trip for banks, spreading, donkey boiler and kitchen	= 12,711

Memorandum of trips, coal and time from F. H. Forbes, G. F. A., O. C. S. B. Co.

Compound Engine, 14 trips, between New York and Fall River, May 15 to June 2. Average time, 11 hours $12\frac{6}{10}$ minutes. Coal 20·65 tons.

Simple Engine, 12 trips, between New York and Fall River, June 4 to 10. Average time, 11 hours $57\frac{6}{10}$ minutes. Coal 27·42 tons.

Deducting 3 tons per trip for banking, spreading fires, donkey boiler and kitchen, all of which is included in the amount of coal given above, makes the actual consumption of coal per trip while engine is running, for Compound Engine 17·65 tons, and for Simple Engine 24·42 tons.

REPORT OF THE BOARD OF EXPERTS ON STREET PAVING.*

The undersigned respectfully submit the following opinion and report on the subject of street pavements in Philadelphia.

The ordinance, approved June 13, 1884, under which you have requested our opinion on this subject defines the purpose of our investigation in the following words, viz:

"SECTION 1. *The Select and Common Councils of the City of Philadelphia do ordain*, That for the purpose of obtaining professional advice on the subject of paving the streets of Philadelphia, the Mayor is authorized to obtain from three engineers, distinguished for their knowledge and experience of pavements, a written opinion and report concerning the subject of pavements in Philadelphia, which report and opinion shall be by him transmitted to Councils for their action."

"SECTION 2. The said opinion and report shall be based upon a careful examination of the present condition of the streets of Philadelphia, and shall point out the defects of the present system of paving, and shall specify what, in the opinion of thesaid engineers, is the best system of pavements for Philadelphia, due regard being paid to durability, economy, smoothness, cleanliness and freedom from noise, and shall further contain an estimate of the cost of such system or systems as they shall recommend."

In order to ascertain "the defects of the present systems" we have made a careful examination of the streets, in connection with data furnished to us by the Chief Commissioner of Highways. We find that the present condition of the streets is as follows:

Paved streets	573 miles.
Macadamized streets	44 "
Unpaved streets	443 "
Total	1,060 miles.

* Report of the Board of Experts on Street Paving to Hon. William B. Smith, Mayor of Philadelphia, July 8, 1884.

On the paved streets the character of pavements is as follows:

Cobble	9,113,925 sq. yds.	93 per cent.
Granite blocks	654,148 "	$6\frac{2}{3}$ "
Asphalt	25,396 "	$\frac{1}{3}$ "
	9,793,469 sq. yds.	100 per cent.

We find further that the cobble pavements, forming ninety-three per cent. of the total are laid upon a bed of loamy gravel without any sand, that the size of the cobble stones is very irregular, varying from three to fifteen inches in size, and that the system of repairs consists in farming out certain districts to the lowest bidder, who contracts to keep the district in good order during the calendar year for a lump sum.

We find in regard to granite block pavements, sometimes called Belgian, that the blocks are laid upon a bed of gravel similar to that above described, that the joints are not cemented, that different qualities of granite have been used with different results, and that these pavements have been greatly injured by being frequently torn up for underground pipes and are not properly relaid.

We find in regard to asphalt pavements that a certain amount of sheet asphalt has been laid in accordance with what is generally known as the Washington Specifications, and that this pavement looks well, although it has not been laid a sufficient length of time to properly judge of its character; and, also, that asphalt block pavements have been laid in small amounts here and there, some of which look very well, and some, as on Sixth street, near Walnut, very badly.

We also find in regard to the general character of the streets in Philadelphia that they are unusually narrow for a city of its size, the prevailing total width between houses being either fifty or sixty feet, giving only twenty-six and thirty-four respectively for the roadway between curbs; and we also find that these narrow streets are used to an exceptional degree for street railroads, the total length of which amounts to two hundred and three miles, and that where these railroads exist, the carriage-way on each side is reduced to a space of only ten or twelve feet in width, which results in confining the travel in such a manner that vehicles follow each other in lines almost as distinctly marked as the lines of the railroad tracks. These exceptional circumstances have a very important bearing upon the character of pavement to be adopted to meet them.

We find that the macadam is used principly in the suburban sections of the city, such as Germantown, Chestnut Hill, etc., where railroad tracks are not so numerous; although in one instance this class of pavement has been used in the heart of the city (part of Twenty-second street) alongside of railroad tracks. The macadam is usually about ten inches deep, composed principally of a blue gneiss found in the vicinity. Its repairs are farmed out on the same principle as the repairs of the cobble pavements.

Upon the condition of facts above described, we have no hesitation in expressing the following opinions:

First.—That the cobble pavement is the most inferior and the most dangerous to health of all pavements made of stone, and is entirely unfit for use in a large city. It is impossible to maintain its surface in an even condition, because the stones have an uneven bearing, owing to their irregular shape, and its joints are the receptacle for large amounts of street filth, which cannot be removed from them, and which give forth, under the varying conditions of heat and moisture, a variety of vapors and odors which are injurious to health. As it is probable, however, that for financial reasons a certain amount of this class of pavement will have to remain in Philadelphia for some years to come, we are of opinion that its evils can be greatly mitigated by a change in the methods of repairs as follows:

In making repairs the stones should be broken to as nearly as possible a uniform size, and no stones should be relaid whose least dimension is less than four inches or greatest dimension is more than seven inches, and they should be laid with the greatest dimension vertical.

The red gravel on which the stones are now placed, which we find by actual test to contain about sixteen per cent. of clay, should be used as a sub-foundation only, being spread to the depth of six inches, and thoroughly compacted by ramming or rolling. On this should be spread a layer of clean, sharp, river-washed sand, free from clay, dirt, or impurities of any kind, and in size not less than one-thirtieth of an inch. This sand should be at least four inches in thickness, and will make the only proper bed for the stones. After these have been set, clean, washed pebble should be raked over the surface to fill the joints, and the pavement should then be thoroughly rammed with a heavy rammer.

In making these suggestions, we desire to state, that we recommend that no more new cobble-stone pavements be laid under any circum-

stances, but we think that if these suggestions are carried out, the existing cobble-pavements can be kept in better order than at present, until such time as they can be replaced with pavements of proper character; and we think that this can be accomplished at less cost than at present, when in consequence of using poor material, the same street has to be relaid two and even three times in the same year. We also think that the system of farming out the repairs by the year is injudicious, because the contract must necessarily lack that element of distinct specification which is the basis of all good work under contracts.

In our judgment it would be much better to make contracts for repairs, by the square yard of work actually done according to exact specifications, the localities where the work is to be performed, to be pointed out by the proper city official.

Second.—In replacing the cobble-stones, or in laying new pavements, we advise that your choice be restricted to one of the following classes, viz: granite blocks, sheet asphalt, or in exceptional cases, asphalt blocks, the selection among these three being determined by the character of travel, and other conditions which will hereafter be referred to. All of these pavements are perfectly water-proof, which is an indispensable quality of a good pavement.

We are aware that at the present time large amounts of wooden pavements are being laid in London and Paris.

As laid in those cities, upon a foundation of concrete of Portland cement and with blocks of seasoned wood especially treated with preserving compounds—they undoubtedly make a more durable road covering than the wooden pavement which proved so signal a failure in this country several years ago. But according to information received very recently from the French Engineers, it is expected to renew the wood every four or six years, depending on the amount of travel. The first cost of the pavements now being laid in Paris is equivalent to about four dollars per yard, and the contract price for repairs and maintenance is forty-nine cents per yard per annum, the contract being made for eighteen years. This brings the total cost during eighteen years to twelve dollars and eighty-two cents, which is about three times the cost, during the same period, of the pavement which we recommend. On account of its cost, both for construction and maintenance, we do not recommend the wooden pavement for the City of Philadelphia.

We advise that the granite block pavement be used exclusively on all streets where the distance between rail and curb (or, if there be no railroad, between curb and curb) is less than twenty feet, also on all streets in the business section of the city where the travel is unusually heavy—say more than fifty trucks or other heavy wagons per hour, and on all streets without regard to width or traffic where the grade is steeper than three in one hundred, except those streets where asphalt blocks are admissible, as hereinafter specified.

We advise that sheet asphalt be laid in spaces near public buildings where quiet is particularly desireable, provided the space is sufficiently wide (more than twenty feet) and the grade not too steep (less than three-one-hundredths) also on streets which are occupied exclusively by dwelling houses and where there are no railroads. As instances of such localities we would cite the space around the new Public Buildings, and on Broad street, the north end of Broad street, Twenty-first street south of Market, portions of Locust street in the vicinity of Rittenhouse square, Diamond street, and other streets of the same character on which there are no railroads. In laying asphalt along railroad tracks, in broad spaces like that surrounding the new City Buildings, a space not less than one foot in width next to the track should be paved with granite blocks, the ends of the blocks toothing into the asphalt.

The asphalt block pavement is somewhat inferior in wearing qualities to the sheet asphalt, but it has great advantages in being cheaper, and in requiring no special plant or skilled labor to lay it. It is well adapted, provided the blocks are properly made, to streets of light travel, occupied solely by residences; but it is proper to state that there is great difference in the quality of these blocks, as ordinarily made, and corresponding risk in adopting them. For heavy traffic they have usually proved a failure. This pavement would be suitable for many of the residence streets of West Philadelphia, but it should not be used on streets where there are railroad tracks, or where the grade is steeper than four-one-hundredths. For the suburban streets of Germantown, Chestnut Hill and similar sections, we recommend that macadam continue to be used, at least until the travel on them becomes very much greater than it is at present.

Having thus indicated the general character of the pavements and the conditions which govern the selection and use of each, we will now speak more in detail of the manner in which they should be constructed and laid.

THE GRANITE BLOCK PAVEMENTS

Should be laid on a foundation of concrete six inches in thickness; or upon a bed of strong clean gravel of the same thickness, formed to the cross-section of the street, and thoroughly compressed by rolling with a steam roller such as is hereafter described, and all subsidence made good by additional material. In case the blocks should be bedded in three inches of sharp washed sand, free from clay or other impurities; the joints should be filled with a preparation of clean, dry pebbles, (heated if laid in damp weather) and the coal tar product known to the trade as No. 5.

In regard to the foundation, we are of opinion that for ordinary traffic the concrete is unnecessary, provided the work is properly done. Well compacted dry earth is an excellent foundation, provided it is kept dry; and if the joints are properly filled with tar and gravel the pavement becomes absolutely water-proof, and no settlement can occur. If, however, the work is not properly executed and the joints not properly filled then water will penetrate to the foundation, and settlement and inequalities will occur. There can be no doubt that the concrete foundation provides against all contingencies. Its use increases the cost of the pavement by about eighty cents per square yard. On streets of the heaviest travel it would doubtless be best to incur this extra expense, although, as previously stated, we do not consider it necessary where such an earth foundation as we have described is kept perfectly dry, and the traffic is not very heavy.

In regard to the quality and size of the blocks, we recommend the Quincy granite as a standard, and all blocks should be equal to this in toughness and hardness, should be of uniform texture, and free from weathering.

Hard, basaltic stone, that will take a polish under travel, should be rejected, as should also the softer quality of granite, and all other gneiss and other laminated stones. The blocks should not be less than three or more than four and a half inches in thickness, six, seven, or eight inches thick, according to the amount of travel, and in length about one and a quarter times their depth. A pavement with narrow blocks affords a firmer footing to the horse, and is less noisy than one with wider blocks; but the thickness cannot be reduced below three inches without impairing the strength of the block. Variations in

length may be permitted to the extent of two or even three inches, but in depth the blocks should be as nearly as possible uniform; no variation greater than one-half inch being permitted. It is impossible to lay a good pavement with blocks of unequal depths, for under travel the blocks will find a level bearing underneath, and all the inequalities will be on the surface. The blocks should be carefully sorted as to width, and those of the same width be used in the same portion of the street. Each block should be regular in shape, with rectangular edges and smooth faces, having no projections nor depressions greater than one-half inch. We advise that the blocks be laid invariably with tar-cemented joints, so as to make the pavement water-proof. No open-jointed pavement should be laid in a city, on account of the large spaces between the blocks giving access into the soil below for the urine and other foul waters of the street, and giving out fumes not only to the street in summer time, but, through heating and ventilating systems, to the interior of houses in winter. With blocks six inches deep and regular in shape, so that the joints will not be too wide, about three and a half gallons of tar will be required to the square yard, and the cost of this is about twenty-five cents. The advantages of the water-proof joints are fully worth this price.

We recommend that the city purchase the blocks which it may intend to lay several weeks or months in advance, so that they may be carefully inspected on delivery, and all blocks which do not conform to the specifications can be rejected. Separate contracts can then be made for laying the blocks after they have been inspected. We estimate the cost of granite block pavements, laid with tar joints in the manner we have described, as follows, per square yard.

	6 in. deep.	7 in. deep.	8 in. deep.
Cost of blocks	$1 80	$2 00	$2 25
Cost of laying	80	85	90
Total	$2 60	$2 85	$3 15

From these prices not less than fifteen cents should be deducted, if the old cobble now on the streets is to be given to the contractors, and 80 cents should be added to them if a concrete foundation is used.

ASPHALT PAVEMENTS.

Under the name of asphalt, a large variety of mixtures have been laid as pavements, but all those which had any value at all belonged to one of three classes, viz:

1st. The largely asphalte comprime, which is largely used in Paris and other European cities. This is made from an amorphous limestone, naturally and intimately impregnated with bitumen, found in the Vosges Mountains, and in Sicily and Hanover. It is reduced to powder, heated, placed on a concrete foundation, and then thoroughly compressed by ramming and rolling.

2d. The American asphalt mastic, which is the standard pavement at Washington. This is an artificial compound, made of refined Trinidad bitumen, powdered stones, and sharp sand. The mastic is placed on a concrete foundation when hot and plastic, and thoroughly compressed by rolling.

3d. The coal-tar concretes, improperly called asphalt, in which the foundation was of broken stone and coal-tar, and the top surface of sand, or fine pebbles, and coal-tar, made into a mastic, spread and rolled substantially as above.

All three of these classes of pavements, when manufactured with skilled labor and laid with proper care, have resulted in good roadway.

The coal-tar pavement is not recommended by us, for the reason that the cementing substance is a product of gas-tar, obtained by interrupting the destructive distillation of that compound, after it has reached a certain point. When placed on the street and subjected to atmospheric influences, a slow and gradual oxidation takes place, by which the tar loses its cementing qualities and becomes inert. The particles of sand then lose their cohesion, and the pavement rapidly disintegrates.

The price of the asphalt comprime in this country at present would not be less than three dollars and fifty cents per square yard. The American asphalt mastic can be laid at a profit for two dollars and twenty-five cents. The asphalt comprime is at times very slippery—an objection to which the American pavement is less subject. There has not yet been sufficient experience as to the comparative cost of repairs of these two classes of pavement to state definitely which is the more durable. Therefore we recommend, at least until further experience, that the American asphalt be used in Philadelphia, in such localities as we have indicated.

A foundation of concrete of the best American hydraulic cement, six inches in thickness, is indispensable for any asphalt pavement. It

is, in fact, the pavement itself, the asphalt surface being merely a cushion to break the force of the travel.

The proper preparation and laying of both the asphalt comprime and mastics is so largely a matter of expert manipulation, besides the necessity for purity in the material, that rigorous specifications are liable to be used by an incompetent or dishonest contractor as an excuse for bad work. In order to obviate their difficulty, it has been the practice for several years in London and other European cities to let contracts for this class of pavement only on satisfactory bond or deposit for maintenance during a period of not less than fifteen years at a specified rate per square yard per annum, the amount of subsidence or dilapidation calling for repairs being rigorously specified, so that it can be clearly understood by the residents on the street as well as the parties to the contract. This system, although it has never been introduced into this country, has met with such uniform success abroad that we recommend it as worthy of the most careful consideration and a full trial.

We recommend that no proposition be entertained looking to the covering of the cobble stones with asphalt or any other compound. Such work will result in nothing but failure, and will create a nuisance on the streets. The reason of this is that the asphalt compound, having no transverse strength of its own, requires an absolutely rigid and unyielding foundation. Cobble stones do not fulfil this condition.

ASPHALT BLOCKS.

These blocks are made of Trinidad asphalt and small particles of lime-stone, mixed together in certain proportions and pressed in a mould under heavy pressure. They are ready for laying as soon as received from the factory. The size of block usually manufactured is twelve inches long, four inches wide, and five inches deep, and we see no reason to change these dimensions. In regard to its foundation, the same remarks apply as already made in regard to granite blocks, *i.e.*, if the foundation is well compacted and perfectly dry, a concrete bed is unnecessary; but the concrete prevents the chance of poor work. The blocks are of such uniform size that the joints are extremely narrow, and the asphalt on the sides soon unites so as to make the pavement water-proof. Asphalt block pavements can be laid at a profit for two dollars and ten cents per square yard on a foundation of gravel and

sand. From this should be made the same deduction for old cobble, and the same addition for concrete foundation, as previously noted.

MACADAM.

In laying new macadam roads in the suburban districts, we recommend that, if the ground be at all springy or damp, a bed of sand or of blast furnace slag, six inches in thickness, be laid as a foundation, and thoroughly rolled.

This will provide most effectually for the drainage in the wet season. The macadam proper should be ten inches in thickness, spread in two layers, each of sufficient thickness to be five inches thick when thoroughly compressed. This compression should be obtained by a steam roller weighing not less than ten tons, and employed for not less than ten hours on each one thousand square yards of each layer. As the use of heavier rollers than can be drawn by horses is necessary for the best type of macadam roads, and as the expense of purchasing a steam road-roller will deter contractors, without assurance of more than one season's work, from bidding on macadam where such purpose is necessary, we recommend the purchase by the city of three rollers, weighing not less than ten gross tons each and not less than two tons per foot run. We estimate the cost of such rollers at about five thousand dollars each. These rollers can be used for compacting foundations for pavements, as mentioned above, as well as for macadam roads, being rented to contractors under proper regulations. In the spring, when the roads are soft, it will be found advantageous to go over all of them, rolling down any projecting stones and bringing the surface to proper shape with fresh materials where any depressions occur, without waiting for the road to be worn out. This, on roads of light travel, with cleaning and watering, will probably be sufficient until fall; on roads of constant and heavy traffic, a roller and store of broken stone should at all times be at the disposal of the engineer in charge, so that the surface can be kept perfect.

For new roads, the bottom layer, which may be of gneiss or limestone, broken to pass through a four-inch ring in their largest dimension, should be placed on a thoroughly compacted bed, as before specified. In rolling this course as above specified, a watering cart and trap screenings should be on hand for use in case the stones begin to roll on each other. When thoroughly compacted, the top course, which should be of tough trap-rock, broken to pass over a one and a quarter inch

hole and to pass through a two and a half inch hole, and free from flat pieces or spaulls, should be rolled with the same precaution as two water carts and screenings, as above, till thoroughly compacted. When fine, dry screenings should be slowly spread, and rolled in until all interstices are thoroughly filled and a coating of powdered screenings one-quarter inch thick remains on top of the stone, when the road should have a plentiful supply of water from a sprinkler directly in front of the roller, which should be kept in motion till all the surface behind it is puddled or has a mottled appearance, when a light coat of screenings will finish the road, and after a few hours drying it can be thrown open to traffic.

The intention of this is to, first, afford a cheap foundation, and, secondly, to make a hard, compact, and, as nearly as possible, homogeneous wearing surface. For this reason trap screenings were specified in the lower coat, as softer screenings, if allowed on the ground, will be worked into the top coat. Any soft material used as "binding" will reduce the cost and labor of rolling, but will make a softer, dirtier, and more perishable road. On country roads a superior economy, resulting from the use of clay or other soft material, may justify it, but on suburban roads all soft material should be rigorously excluded.

A macadam road should be maintained by thoroughly wetting any depression and rolling in stones from one to one and a half inches in size, in order that general repairs, except on heavily traveled roads, should seldom be necessary. The dust and debris of a macadam road should not be swept off in dry weather, unless water enough can be afforded to make a very liquid mud. When that is attained, naturally or artificially, a soft revolving broom should be used, with such draft that the heavy debris is not removed. When there is an inconvenient amount of trash on the road, the same broom, with a lighter draft, can be used to remove it. The road should not be scraped, and should be let alone when the mud is not sufficiently fluid.

We estimate the cost of a macadam pavement, laid in the manner we have described, at one dollar and seventy-five cents per square yard.

In conclusion, we submit the following recommendations of a general character :

All contracts for pavements (excluding macadam) should contain a clause by which the contractor guarantees to keep the pavement in good repair for a period of not less than five years from the date of its acceptance by the city, and to insure a compliance with this guarantee,

the city should retain ten per centum of its cost until the expiration of the five years term.

No pavement, no matter how well laid, can be kept in proper order if it is constantly disturbed for the purpose of laying or repairing underground pipes. In order to avoid this as far as possible, all drains, gas, water, and other pipes, should be laid, or if already laid should be thoroughly repaired, before the pavement is put down. Such openings in the street as subsequently become unavoidable, should be made only by permission of the city officials in charge of the streets, and under their direct supervision; and before such permits are granted, the applicant should be required to pay the estimated cost of repairing and relaying the pavement, and these repairs should be made under the immediate direction of the same official, who should be held responsible that they are made properly.

The crown or transverse section of the pavement should be the arc of a circle, with versine about one-eightieth of the chord, or say for

Streets 26 feet wide, crown 4 inches.
Streets 34 feet wide, crown 5 inches.
Streets 50 feet wide, crown 7 inches.
Streets 70 feet wide, crown 10 inches.

In wider streets these may be slightly reduced when the longitudinal slope exceeds three one-hundredths. The distance from top of curb to surface of pavement at the curb should be between five and seven inches, depending on the width of the street.

In cases like that of Locust street, between Sixteenth and Seventeenth streets, where the ground is soft and full of springs, we would advise that "blind" or broken stone drains be laid under the foundation, these drains being connected at their lower ends with drain pipes leading into the nearest sewer or inlet. A concrete foundation should be laid on all such streets as a further protection against settlement. No sewer pipe intended to carry house drainage should be laid with open joints for the purpose of draining the soil. Such pipes would speedily pollute the soil and produce sickness.

The ordinance under which we have made this report requires that the report shall "contain an estimate of the cost of such system or systems as we shall recommend." We have already stated that the cost per square yard of the standard pavements which we recommend, is as follows:

7 inch granite blocks on concrete feundation,	$3 65
6 inch granite blocks on gravel foundation,	2 60
Sheet asphalt on concrete foundation,	2 25
Alphalt block on concrete foundation,	2 90
Asphalt block on gravel foundation,	2 10

Although the existing granite block pavements are not cemented in the joints, yet they are so much better than the cobble pavements, that it is not likely that any plans for relaying them will be considered at present. This leaves the 535 miles of cobble pavements, containing in round numbers 9,000,000 square yards. Of this a length of 203 miles is traversed by street railroads, between the rails of which we would advise that the cobble be allowed to remain for the present. This reduces the area by about 600,000 yards, leaving 8,400,000 yards to be repaved. It is not probable that more than 80 miles of streets, say 1,400,000 yards—will fulfill the conditions which we have named as limiting the use of asphalt pavements, and of this not more than one-fourth to one-third should be asphalt block.

We have then seven million yards remaining for granite blocks, and of this perhaps one million yards in the heavy business streets, should be deep blocks laid on concrete.

This gives the data for the following approximate estimate, viz.:

1,000,000 square yards granite block a.,	$3 65	$3,650,000
6,000,000 square yards granite block a.,	2 60	15,600,000
1,100,000 square yards sheet asphalt,	2 25	2,475,000
300,000 square yards asphalt block,	2 10	630,000
8,400,000 square yards,		$22,355,000

From this should be deducted the value of eight million four hundred thousand yards of cobble, at fifteen cents, one million two hundred and sixty thousand dollars, leaving the net cost twenty-one million and ninety-five thousand dollars.

It is estimated that two million dollars could be advantageously expended in each year, in laying about fifty miles of improved pavements so that the cobble stone would be entirely replaced in between ten and eleven years.

These estimates are necessarily general and approximate only. They are given merely for the purpose of stating what will be the probable ultimate cost of replacing the cobble stones. Our estimate of prices per square yard is based upon the actual cost of similar work in

other cities during the past few years, and it may be relied upon as reasonably exact, at the present prices of labor and materials.

Respectfully submitted,

(Signed) Q. A. GILLMORE,
F. V. GREEN.

The undersigned agreeing with the above, wishes to add a recommendation that in the case of all pavements except those laid on concrete when the ground is wet or soft, a layer of fine sand or blast furnace slag be placed or rolled under the bed of gravel specified, with the purpose of preventing the subsidence of the gravel into the clay.

Also, for the lanes or alleys in the old part of the city, where water is discharged into the gutters, the use of rock-asphalt mastic made from either one of the following brands, viz.: *Val De Travers or Neuchatel*, *Seyssl*, or the mastics of the *United Limmer Company*. These mastics when properly laid are not disintegrated by hot or other water, and can be laid so hard that wheels following in one track will not injure them. This pavement can be laid on six inches of concrete, one and one half inches thick, for two dollars and seventy-five cents per square yard, and the gutters will be so smooth that water will not lodge in them as at present.

Very respectfully,

(Signed) EDWARD P. NORTH.

INFLUENCE OF MAGNETIC PERTURBATIONS UPON SCINTILLATION.—Montigny has published a series of observations, from which he draws the following conclusions: 1. The intensity of scintillation upon the day of a magnetic storm is greater than that upon the preceding or upon the following day, provided those days are free from magnetic disturbance. 2. The increase of scintillation is very marked when the disturbance comes on or persists at the very moment of observation. 3. The path described by the image of the star in the telescope is generally less regular on the days of disturbance than on the previous or on the following day, provided the atmospheric conditions are the same. 4. The increase of twinkling during periods of drought and of magnetic storms is equivalent to the excess of the increments of twinkling, under the influence of magnetic disturbances which arise during rainy periods. In connection with his observations, Montigny refers to the experiments of Henri Becquerel and M. Jubert, upon the influence of terrestrial magnetism on the plane of polarization of a ray of light traversing a tube filled with bi-sulphide of carbon and placed in the magnetic meridian.—*Bul. de l'Acad. de Belg.*, 1883. C.

SURVEYS FOR THE FUTURE WATER SUPPLY OF PHILADELPHIA.

By RUDOLPH HERING, C. E., *Assistant in Charge.*

(Continued from page 152.)

The lines for the proposed conduit to the Perkiomen were run as follows:

Starting at the site of the proposed Cambria reservoir, the line follows the best course to the Wissahickon creek, which it crosses near the old log cabin. Following the west bank for about five miles, with several short tunnels, it reaches a point about one mile north of Barren Hill, where two alternate lines diverge. One continues up the Wissahickon valley; the other runs towards Norristown.

The latter, through a short tunnel, reaches the Plymouth creek, crosses the Plymouth Railroad overhead, continues through Cold Point and the northern part of Hickorytown to a point about two miles northeast of Mageetown, where a short tunnel brings it into a small valley; then, with another tunnel, it reaches Saw Mill Run, at the northeast corner of the borough of Norristown. From Hickorytown an alternate line to this point shortens the distance, but increases the length of tunneling. The line then crosses the Norristown ridge with a tunnel into Stony Brook valley, crosses the railroad overhead, and then runs to a point one and a half miles southwest of Norritonville, whence a tunnel, about three miles in length, carries it into the Skippack valley. The width of this valley at the most favorable point for crossing is about two thousand one hundred feet. Passing Skippackville, the line reaches a point near Amityville, whence two tunnels bring it into the valley of the Northeast Branch of the Perkiomen, about one mile southwest of Lederachville. Crossing this stream, and following its western bank to near Branchville, it passes through another tunnel, and reaches Sumneytown and the proposed storage dam. From a point about one mile north of Lederachville, an alternate line was run directly to the Perkiomen at Salford Station, requiring a shorter tunnel.

The alternate line, diverging near Barren Hill, follows the west side of the Wissahickon to a point near Gwynedd Station, thence to Towamencin Creek with a five mile tunnel, which, if a higher grade is adopted, can be reduced to two and a half miles, and finally joins

the line before mentioned, near Lederachville, after crossing the Skippack two miles northeast of Skippackville, and passing through another tunnel.

Topography was extended sufficiently far to examine still other alternate lines. Only one line remains to be run from a suitable point on the present line to opposite Schwenksville, and thus complete all practicable locations for a conduit into the Perkiomen basin on the proposed grade.

A careful survey was made of the entire country to be flooded by the proposed dams at Schwenksville, Green Lane, and Sumneytown, and the general topography of the water-shed of all the ground west of the Perkiomen and of the Macoby was taken, up to the county lines where it joins the territory covered by the State Geological Survey.

Mr. Linton was assisted in this work by

George B. Mifflin, transitman, appointed June 12, 1883.
Kenneth Allen, leveller, appointed May 30, 1883.
R. T. Vaughan rodman, appointed May 80, 1883, left December 22d.
H. A. Schofield, rodman, appointed May 28, 1883.
Amasa Ely, chainman, appointed May 28, 1883, left September 6th.
W. E. Parker, chainman, appointed September 10th.
Max Atlee, flagman, appointed May 28, 1883, left December 22d.
George W. Wood, axeman, appointed June 4, 1883, left December 22d.

(*b*.) *Lehigh Party*, Mr. A. P. Berlin, C. E., in charge.

The object in view in making an examination of the Lehigh watershed was simply to collect enough information to have a reasonably fair knowledge of its value in connection with the Perkiomen scheme, and to ascertain the feasibility of a conduit discharging into the Perkiomen basin. No detailed surveys were contemplated or made.

Mr. Berlin, of Easton, Pa., being familiar with the country, was entrusted with the duty: First, of collecting all existing maps and information that would assist in the work; and Secondly, of making a reconnaissance of the entire water-shed, noting its general physical and topographical features, in order both to correct and to supplement existing maps.

He began his work July 19th, and completed it September 4th. He was assisted from July 23d to August 17th by Mr. C. P. Bassett.

The maps that were procured and copied are as follows:

1st. Lehigh Valley Railroad from the Summit above White Haven to Allentown, with 20 feet contours extending some distance from the line.

2d. Warrantee Tract Map between the Lehigh Valley and the D. L. & W. R. R., and between the Lehigh and Susquehanna Rivers.

3d. Profile and general topographical features of Bear Creek Valley.

4th. The township maps of the counties situated in the water-shed under consideration.

Mr. C. E. Stedman, Chief Engineer L. V. R. R., rendered valuable assistance in securing and loaning existing maps, etc.

The field work consisted in reconnoitering the territory which drains into the Lehigh at White Haven and in recording about 500 aneroid elevations. But few sights and angles were taken, as it was not deemed necessary for the present inquiry to spend the time for an accurate survey. The Luzerne county map was found to be quite reliable; the Monroe county map fair; and the Carbon county map very inaccurate. Contours were sketched every twenty feet over the water-shed in the following townships: Tobyhanna, Coolbaugh and Tunkhannock, in Monroe county; Buck and Bear Creek, in Luzerne county; Dennison and Covington, and a portion of Kidder and Penn Forest, in Carbon county. The elevations were reduced to sea level.

The topography as thus obtained gives a fair idea of the territory for the purposes of ascertaining the area of the water-shed, as well as its general features for collecting and storing water.

The area which may be utilized for the water supply of the city is almost entirely covered with forests in different stages of growth. The streams furnish a very uniform flow, comparatively, and the water is excellent throughout. The main tributary to the Lehigh is the Tobyhanna river in Coolbaugh township, draining several lakes and ponds. The Tunkhannock and Trout creeks are small but steady streams which discharge into the Lehigh on the south side: the Bear, Wright's Shades, Cloke, Cold Spring, and Pond creeks empty into it from the north. The entire water shed is from 1,100 feet to about 2,000 feet above the sea, too high ever to be much used for agriculture. The soil, furthermore, is everywhere coarse and stony.

A conduit bringing the water down the valley would be situate on the left bank, at least as far down as below Mauch Chunk, and intercept all the streams crossed. It would then follow down through the Lehigh Gap, continue near the river to Siegfried's Bridge and then

leaving it, cross the country towards the Perkiomen. A tunnel of less than four miles would permit the Lehigh water to discharge into the Perkiomen basin near Hossensack, at an elevation of about 400 feet above city datum.

The great fall available for the conduit would permit its diameter to be smaller, and thus its cost may compare favorably with that of other schemes requiring less length of conduit.

(*c.*) *Delaware Party*, F. L. Paddock, C. E., in charge.

The field work commenced June 6, with a small party, but from June 12 was continued with a full corps until December 17, or for 167 working days, when office work was begun.

During this time 305·68 miles were measured with steel tape, of which 113·42 miles were for conduit lines; 317·1 miles were measured by stadia and gradienter; 1,558 vernier angles were turned; 6,743 magnetic bearings were taken with the transit; 183·7 miles were leveled with the "Y" level and 272 bench marks were established.

The areas covered by the surveys were

23	square miles for conduit lines,
1·5	square miles for storage basins, and
20	square miles for general water-shed.
44·5	square miles.

Four and two-thirds months were devoted to the survey of a conduit line to the Delaware Water Gap, which was run as follows:

Two alternate lines were located from the city to Huntingdon Valley, beginning at the city line near Second street, west of Tacony Creek. One runs to Fox Chase, and thence to the junction of Harper's Run with the Pennypack creek and along the west bank of the latter to Huntingdon Valley. The other, longer in distance but requiring less expensive work, extends in a northwest direction to the left of Ashbourne on the North Penn R. R., strikes the Tacony creek near Shoemakertown, and thence follows the west bank of this creek to its head near Jenkintown. From here it follows down Paul's Brook to the Pennypack creek, where it joins the other line near Huntingdon station.

The conduit line then extends up the Pennypack, on its west bank, to the Forks, whence again several alternate lines were run: one through Johnsville to the west of Jacksonville, striking the Little

Neshaminy at Rosse's Mill; another follows up the Eastern Fork of the Pennypack to Danville, thence to Nippe's Run, back of Jacksonville, and down the latter to the Neshaminy; and a third passes Hartsville station near the head of the Warminster Creek, and follows the same to its junction with the Little Neshaminy. The country over which these lines run is the dividing ridge between the Pennypack and Neshaminy waters and will require a tunnel about two miles in length.

From Rosse's Mill the line follows the west bank of the Little Neshaminy, and crosses the Big Neshaminy west of the Forks. Thence it extends up Mill Creek to Lahaska Creek, and follows the latter to Greenville. Here another dividing ridge requires a tunnel $2\frac{1}{2}$ miles in length, which brings the conduit line to Carversville.

An alternate line was run in a northeasterly direction from near Vandegroft's Mill to the Trout Ponds or Ingham Springs, near New Hope, passing up one of the branches of Pidcock Creek to Clayton, and through a gap between the Solebury and Buckingham Mountains, down to the Springs.

Another line was run from the head waters of Pidcock Creek, to Glendale, whence it was intended to extend it by a short tunnel through the Solebury Mountain, to Aquelong Creek, and thence to New Hope.

From Carversville the first line follows the valley of the Paunaucussing Creek down to the Delaware river at Lumberville, and continues on or near its western bank up to the Water Gap.

About one and a third miles above Lumberville, and at the mouth of the Tohickon Creek, it reaches Point Pleasant, where the conduit might temporarily terminate. The distance from the Frankford Reservoir to this point is thirty miles.

Contrary to expectations gained from previous reports, the above line as far as Point Pleasant is found to compare well with the lines to the Perkiomen. The length of tunneling is much less than was supposed, and there are but but few valleys to cross. No detailed comparison has as yet been possible, except to observe that their length and the amount of tunneling necessary are about the same.

After crossing the Tohickon Valley the conduit would follow the bank as far as Tinicum Creek. From there it can run either in a direct line to a point west of Kintnersville, about a mile from the river, cutting off an extensive bend in the same by a five-mile tunnel, pro-

bably mostly through trap rock, or it can follow the river along a very steep bluff through sandstone and slates. Above Kintnersville the conduit runs inland as far as Monroe. At Easton it crosses the Lehigh river, follows along Second street, and then crosses the Bushkill Creek, all by means of one inverted syphon 3,900 feet in length. From here to the Gap the line is not troublesome.

The party devoted the remainder of the season to running a tie line from Point Pleasant, on the Delaware, to Schwenksville, on the Perkiomen, to connect the levels and lines of the two parties, also to surveying sites for storage basins in the Tohickon Valley, and to crossing the same with a few base lines from which to take the topography during the following season.

Mr. Paddock was assited in this work by

W. T. Forsythe, transitman, appointed June 10th.

E. C. Bull, leveler, appointed June 5th, left December 20th.

Jacob Stadleman, rodman, appointed June 4th, left July 7th.

Isaac Forsythe, rodman, appointed June 4th, left December 20th.

G. S. Cheyney, chainman, appointed June 4th.

Ross Kirk, chainman, appointed July 9th, left November 3d.

C. E. Taylor, axeman, appointed June 18th.

Thomas Jamison, flagman, appointed June 5th, left December 20th.

B. HYDROGRAPHIC WORK.

The hydrographic branch of the investigation was intrusted to Mr. C. S. Gowen, C. E. He reported for duty June 24th, and has since been engaged in ascertaining.

1. The flow of the different streams in the Perkiomen, the Neshaminy and Tohickon valleys;

2. The minimum flow of the Delaware and Lehigh rivers; and

3. The rainfall on the Delaware, Schuylkill, and Lehigh basins in general, and on the Perkiomen, Neshaminy and Tohickon basins in particular.

For the Perkiomen project it was decided to gauge the following streams:

The Perkiomen at Green Lane and Schwenksville.

The East Swamp Creek at Sumneytown.

The Macoby at Green Lane.

The West Swamp Creek at Zieglersville.

The Northeast Branch, one and a half miles above Schwenksville.

The Skippack at Skippackville.

For the Delaware project it was decided to gauge the Pennypack at Shelmire's mills, the Neshaminy at the Forks, and the Tohickon at Point Pleasant.

A careful search was made for proper sites at which to place the gauge posts, and where to build the necessary weirs and to place current meters, so that reliable results could be secured at the least expense.

It has not yet been possible to compute the results of this season's work. It may, however, be stated in general, that the combined minimum, and also the mean flow in the Tohickon and Neshaminy Creeks from July to December 1st was considerably less than in the Perkiomen and its branches, but that in December their combined mean flow was somewhat larger.

This results is no doubt due partially to the fact that the rainfall on on the Perkiomen water-shed was somewhat greater, and also to the fact that the territory about the headwaters of the Perkiomen is more wooded, and therefore prevents evaporation and immediate discharge better than the territory near the head of the Tohickon and Neshaminy water-sheds, which is mostly cultivated land.

From the fact that the many mill privileges cause a very irregular flow by the storing of water and its subsequent rapid discharge, it is extremely difficult to get accurate results. Automatic gauges would obviate this difficulty, and it may be advisable, after the last year's work is compiled, to establish several in both the Perkiomen and Delaware divisions during the following season.

Appended will be found drawings of two types of weirs, as built, one being in soft ground, as on the Perkiomen above Green Lane, and the other on rock, as at the Forks of the Neshaminy.

To ascertain the minimum flow of the Delaware and Lehigh rivers, suitable localities were selected, and meter measurements were taken at the lowest stages of the rivers. It was stated at the time when our measurements were taken, that the river at the Water Gap had not been as low for many years. The minimum flow of the Delaware at that point was found to be 697 million gallons per day, and of the Lehigh, at White Haven, 76 million gallons per day.

The flow of the Schuylkill river has heretofore been approximately measured by ascertaining the quantity of water pumped, the quantity used for the wheels and for the locks, and estimating the leakage, when no water was flowing over the dam. It was, therefore, not deemed necessary at this time to make any further measurements.

The rainfall question next received consideration. Gauges, such as

are used by the United States Government, were placed at every point where it was desirable to measure the precipitation and where no observations had previously been made. In order to ascertain the intensity of the storms, especially during the heaviest fall, and to leave an exact record of their duration, automatic gauges were required. After considerable search for the best instruments, we succeeded in having three made according to the pattern of Prof. Draper, in charge of the Meteorological Station at Central Park, New York City.

The rainfall records of numerous volunteer observers, stationed on or near the water-sheds under consideration, have been collected and are appended. These, together with our own, will, when compared, enable us to determine the amount of precipitation on the Perkiomen, Neshaminy and Tohickon water-sheds. As yet no complete comparison of the results has been possible. The only conclusion which can be given so far is that the rain-fall on the Perkiomen water-shed during the last six months of 1883, was somewhat greater than that on the Tohickon and Neshaminy water-sheds.

The nature of the work of the hydrographic party being such that even one whole season's results cannot lead to a final conclusion, owing to the varying intensity of the seasons during the different years, it is essential that the observations should be continued for several years without interruption. To cease gauging the streams continuously before the final adoption of one of the projects, might result in serious consequences. The absence of a full record might easily, for instance, cause the assumption of an erroneous quantity for the necessary storage capacity on the different streams.

The expense of permanently continuing this work, now that the weirs are built and the guages set, would not be considerable, and certainly would be worth much more than its cost. In connection therewith the rain gauging should likewise be continued, at least for several years. New York and Boston both continue permanently rain and stream gauging connected with their water supplies.

Mr. Gowen was assisted in his work by

H. W. Sanborn,	sub-assistant,	appointed	July 20.
E. S. Crawley,	rodman,	"	June 25, left Sept. 8.
Amasa Ely,	"	"	Sept. 7.
H. C. Shurtleff,	"	"	Oct. 2, left Dec. 11.
B. Franklin,	"	"	July 17, " Aug. 17.
E. S. Campbell,	"	"	Aug. 20, " Sept. 8.
G. A. Luccareni,	"	"	Sept. 6, " Oct. 31.
J. G. Hillsman,	"	"	June 30, " Dec. 15.

PRECIPITATION—IN INCHES AND HUNDREDTHS—1883.

STATION.	Elevation above sea level.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Total for year.	OBSERVER.
Phila. Water Dept. (automatic)	110	...	...	...	...	...	...	...	...	...	3·37	1·22	2·71	...	Phila. Water Dept.
U. S. Signal Service, Phila.	140	4·13	5·04	2·02	2·44	1·91	5·91	1·78	3·40	4·26	4·18	1·34	2·76	39·17	T. F. Townsend.
Pennsylvania Hospital, Phila.	50	3·78	4·79	2·43	2·98	1·95	5·96	2·04	4·11	4·43	4·11	1·56	3·15	41·29	Penna. Hospital.
Moorestown, N. J.	48	4·05	4·77	2·33	3·08	4·70	4·51	1·57	4·07	4·77	5·22	1·48	3·25	43·81	Thos. J. Beans.
Fallsington, Penna	44	4·07	4·60	2·60	3·88	3·34	5·04	2·05	5·36	3·64	4·02	1·56	3·31	43·47	Millnor Gillingham.
Princeton, N. J.	216	3·72	4·40	1·65	2·46	2·40	6·32	1·57	3·60	3·26	3·31	1·48	2·75	36·96	M. McNeill.
Doylestown, Penna	387	...	...	...	...	...	...	...	...	...	3·80	1·43	3·06	...	Phila. Water Dept.
Doylestown, Penna. (automatic)	387	...	...	...	...	...	...	...	...	...	...	1·39	2·21	...	Phila. Water Dept.
Ottsville, Penna	340	...	...	...	...	...	...	...	...		4·71	1·82	4·05	...	Phila. Water Dept.
Quakertown, Penna	536	3·40	3·20	1·69	2·65	2·09	6·98	1·05	2·25	3·72	4·10	1·47	2·15	31·75	J. H. Heacock.
Phillipsburg, N. J.	163	...	3·06	...	1·71	1·86	4·97	4·92	1·44	3·99	4·81	...	...	...	Emily Kent.
Easton, Penna.	328	3·20	3·87	2·40	2·94	2·68	4·62	4·86	2·36	4·27	5·22	1·61	3·36	41·39	Prof. S. J. Coffin.
Mouth of the Perkiomen, Penna	87	3·83	4·28	1·92	3·30	1·84	8·31	3·07	4·85	2·68	3·91	1·70	3·50	43·19	E. F. Smith.
Pottstown, Penna	150	2·82	3·32	1·33	3·45	2·80	5·65	4·29	4·89	4·80	4·64	1·88	1·10	40·97	Chas. Moore.
Zieglersville, Penna	174	...	...	...	...	...	...	...	4·00	3·96	4·77	1·75	5·76	...	Phila. Water Dept.
Green Lane, Penna	363	...	...	...	...	...	...	...	3·20	6·51	5·20	1·69	4·21*	...	Phila. Water Dept.
Green Lane, Penna. (automatic)	270	...	...	...	...	...	...	...	...	...	...	1·43	...	...	Phila. Water Dept.
Pennsburg, Penna	354	...	...	...	...	...	...	...	...	5·24	5·27	1·93	3.31	...	Phila. Water Dept.
West Chester, Penna	459	4·52	5·48	3·62	3·52	2·71	5·72	2·54	5·20	3·64	4·45	2·30	4·63	48·33	Dr. J. C. Green.
Reading, Penna	200	4·42	4·23	3·09	3·05	1·81	6·05	3·83	2·99	3·80	4·20	0·81	2·73	41·01	E. F. Smith.
Lebanon, Penna	498	4·35	4·22	3·70	2·89	2·98	8·54	2·96	2·68	4·46	5·93	1·42	3·28	47·41	S. B. Lehman.
Schuylkill Haven, Penna	513	4·00	3·60	3·13	2·34	3·55	5·24	2·87	1·28	3·34	4·59	1·48	2·99	38·41	E. F. Smith.
Wilkesbarre, Penna	546	4·98	2·38	2·55	2·70	5·28	8·52	7·08	0·88	3·19	3·12	1·45	1·99	41·12	Francis B. Hodge.

* Partially estimated.

LIST OF OBSERVERS ENGAGED BY THE DEPARTMENT.

NAME.		PLACE.
J. G. Hillsman,	since Dec. 15,	Frederick, Montgomery Co.
N. S. Renninger,	" July 24,	Green Lane, "
G. H. Hart,	" Sept. 9,	Pennsburg, "
G. W. Roth,	" Sept. 1,	Oftsville, Bucks Co.
Thos. Walton,	" Oct. 5,	Doylestown, "

In addition to the above, records from the following localities have been furnished, which greatly assist in arriving at proper conclusions, and for which we are indebted to the following parties:

Gen. W. B. Hazen, Chief Signal Officer, Washington.
T. F. Townsend, U. S. Signal Service, Philadelphia.
Mr. E. F. Smith, Chief Engineer Canals of Reading Railroad Company, Reading, Pa., for observations at Schuylkill Haven, Reading, and Browers (mouth of Perkiomen).

Mr. Thos. J. Beans	Burlington, N. J.
Mr. Francis B. Hodge	Wilkesbarre, Pa.
Mr. Charles Moore	Pottstown, Pa.
Mr. S. B. Lehman	Lebanon, Pa.
Mr. Millnor Gillingham	Fallsington, Pa.
Mr. M. McNeill	Princeton, N. J.
Mr. J. H. Heacock	Quakertown, Pa.
Miss Emily Kent	Phillipsburg, N. J.
Prof. S. J. Coffin	Easton, Pa.
Dr. J. C. Green	West Chester, Pa.
Prof. E. Pliny Chase	Haverford College, Pa.
Pennsylvannia Hospital	Philadelphia.

The report of Mr. Gowen on the detailed operations of the party is herewith appended.

PHILADELPHIA WATER DEPARTMENT.

January 23, 1884.

RUDOLPH HERING, ESQ.,

Assistant Engineer in charge of Surveys for a New Supply of Water.

SIR:—Herewith is submitted a report of the operations of the Hydrographic party connected with the surveys for a new supply of water, for the season ending January 1, 1884.

In accordance with instructions I reported for duty at the Water Department, June 24, and after making the necessary arrangements

for field and office work, proceeded, June 26, to Schwenksville, in the Perkiomen division, where headquarters for the party were established for the season.

STREAM GAUGING.

As the Perkiomen division is the most extensive, my instructions were to make a beginning on that side, and accordingly explorations and surveys of all those streams were immediately begun in order to establish gauging points.

It was proposed at first to use as far as possible current meters for gauging, but investigation showed that little could be done to advantage with those instruments under the ordinary conditions of the summer flow of the streams. These natural channels are very rough and of considerable width, showing everywhere the marks of the high flows of the winter and spring months, and the inclination of their beds are, as a rule, considerable, the streams forming, in most cases, a constant succession of mill-ponds, with but short intervals between each dam and the head of the succeeding pond below. Besides, the extreme shallowness of all the streams in their normal summer condition rendered the use of the current meter impossible at points where there was a perceptible current.

Accordingly it was decided to build a number of low weirs or dams, and to measure the flow passing them. This method offered the advantage that the manner of observation is simple, taking but little time, and affording opportunities for continuous measurements whenever desired. The weirs were intended to gauge the ordinary summer or low flows, and the meters were to be used whenever the streams were too high to allow the weirs to act as such.

(*a*). *Perkiomen Division.*—Gauging points were decided upon at the following places in the Perkiomen division :

Perkiomen, above Green Lane.
Macoby, " " "
East Swamp Creek, at Sumneytown.
West Swamp Creek, at Zieglersville.
North East Branch, at Schwenksville.

As it would take considerable time to build the weirs, gauges were placed at the following points:

On the Perkiomen, in Shaw's meadow, above Green Lane.

On the Macoby, at the "foot log," above Green Lane, near the house of Mr. Hildebeitel.

On the East Swamp Creek, at Sumneytown, on the land of Daniel Krauss, below the turnpike bridge.

On the Perkiomen, at Frederick, below the stone bribge.

On the Perkiomen, at Schwenksville, at Longaker's dam.

On the Perkiomen, at Schwenksville, below Longaker's dam.

On the West Swamp Creek, at Zieglersville, below Leidy's bridge.

On the Northeast Branch, at Schwenksville, on the land of John Alderfer.

These gauges are made of inch boards, painted white, and graduated to hundreths of feet. In a majority of cases they were attached to 6″ x 8″ hemlock posts, which were firmly set into the banks of the stream, and in all cases the tops of the gauges were connected by levels with defined bench marks near by, the levels being repeated from time to time as occasion demanded. Regular daily observations of the heights of the streams, began as follows:

West Swamp Creek Gauge	June 27
Longaker's Dam	" 30
Gauge below Longaker's Dam	" 30
Frederick Gauge	July 2
Northeast Branch Gauge	" 7
East Swamp Creek "	" 12
Gauge on the Perkiomen above Green Lane	" 12
Gauge on the Macoby " " "	" 13

As soon as it was decided to use weirs (about July 15) measures were taken to start work upon the Perkiomen weir, on Land owned by Mr. Shaw of Philadelphia, situated at Green Lane, plans having been prepared for that purpose, but on seeing Mr. Shaw, it was learned that he intended to build a dam which would flood our weir location. Finding a new location and making new arrangements occasioned delay, but work was started on the weirs as follows:

Macoby Weir	July 26
East Swamp Creek Weir	" 30
Perkiomen "	Aug. 8
Northeast Branch "	" 9
West Swamp Creek "	Sept. 3

Regular daily measurements of the flow at these weirs began as follows:

Macoby Weir	....	Aug. 8
East Swamp Creek Weir	....	" 8
Perkiomen "	....	" 29
Northeast Branch "	....	" 27
West Swamp Creek "	....	Sept. 13

The Perkiomen weir is located about one and three-fourths miles above Green Lane, on the land of Michael Gettle. Access to the weir is obtained over the land of William Smith. The weir is built in a bed of compact gravel; the length of crest or overflow is 68·83 feet, and the depth of the water on the up stream side is about 17 inches below the crest. The method of construction of this weir, which is the same, except in a few details, as the East Swamp Creek, the Northeast Branch and the West Swamp Creek weirs, is as follows:

Two 8 inch by 10 inch hemlock sills were placed parallel to each other across the bed of the stream, extending on either side a considerable distance into the bank, and forming, when joined together, a "bed" frame about four feet wide. Against the upper side of the upper sill, which was carefully placed to a true line, 3 inch tongued-and-grooved hemlock planks were placed in a dug trench, and then driven to a depth varying from $2\frac{1}{2}$ to $3\frac{1}{2}$ feet below the sills. These planks were spiked to the sill, and against them another 8 inch by 10 inch sill was then placed and firmly bolted to the planks and other sill, with three-quarter inch bolts, at intervals of about $2\frac{1}{2}$ feet. The upper ends of the sheeting planks were allowed to project above the sills, and were stiffened by pieces of 6 inch by 8 inch timber placed against the down stream side, and to the up stream side were fastened the crest pieces, with five-eighth inch lag screws, placed 18 inches apart.

The crest pieces were made of well-seasoned oak plank, two inches thick and eight inches wide, carefully worked to true lines and surfaces, the upper edge, over which the water passes, presenting a horizontal face, one inch wide, with a beveled face for the remaining thickness. The crest pieces project from four to five inches above the timbers below, and their ends are either butted or mortised together. In the former case the joints are covered on the up stream side with thin iron plates, to prevent possible warping or leakage.

The upper face of the bed-frame is covered with two-inch plank, four to five feet in length, forming the apron of the weir. In the

Perkiomen weir this apron was surmounted by another, placed about a foot above, and the intervening space filled with heavy stones and gravel, forming a crib. The apron extends at either end for a considerable distance beyond the ends of the overflow; and the weir was further loaded by filling these parts of the apron with heavy stones. The up stream side of the sheeting, and the banks near the ends of the dams, were well puddled. The sheeting at the ends of the overflows was carried to a height, varying in the different weirs, from one to two feet above the level of the crest, and at that elevation extended into the banks.

(To be concluded.)

Methods in Physical Astronomy.—Physical astronomy is a science which is altogether modern and for the most part contemporaneous. Its foundation was the discovery of telescopes. The telescopic harvest was so nearly exhausted by Herschel that the need of further assistance was generally felt. Arago sought help from polarization and his investigations prepared the way for spectral analysis. The small mass of matter which constitutes the chemical molecule, when it vibrates freely in the gaseous condition, sends out a special system of waves which varies with the chemical character of the molecule. The system of luminous waves may be compared to the system of sounds given by a vibrating cord, a system which depends firstly, upon the length of the cord, secondly, upon the volume, the timber and other circumstances which accompany the vibration. In the preliminary steps of spectral analysis we find the names of Wollaston, Fraunhofer, Sir John Herschel, Talbot, Miller, Wheatstone, Swanmasson and Foncault. Kerschof and Bunsen made a synthesis of all these efforts and brought the method into its present general and practical form. When spectral analysis was presented to the scientific world it held in one hand cæsium and rubidium, in the other a list of metals recognized in a star situated at a distance which is almost inconceivable. The subsequent discovery of telluric bands and the elective absorption exercised by atmospheric vapor prepared the way for the study of planetary atmospheres, which, when it becomes more complete, will show whether our atmosphere represents a type which is everywhere reproduced, or whether the varieties of atmospheric composition will lead us to admit the appearance and the development of life in media which are essentially different. The most recent method of physical astronomy is photographic. It has already brought a marvellous help to scientific study. The first image of a fixed star upon the daguerrean plate was that of the sun. It was obtained by Messrs. Fizeat and Foncault, the authors of the admirable methods of measuring the velocity of light, on the second of April, 1845. During the same year photographic impressions of fixed stars were obtained in the United States, and soon afterwards Rutherfurd and de la Rue produced their beautiful photographs of the moon. Rutherfurd, Gould and Draper

extended stellar and nebular photography and Janssen crowned the work by his solar photographs, some of which resulted from an exposure of less than $\frac{1}{10000}$ of a second. The photographic plates which are now prepared are not only sensitive to all the elementary rays which excite the retina, but they even extend their power into the ultra violet regions and into the opposite regions of obscure heat, in both of which the eye is equally powerless. Well then may Janssen say, that "the photographic plate will soon be the true retina of the savants."—*Lumiere Electrique*, April 12, 1884.

Solar Motor and Solar Temperature.—After experiments extending over twenty years and involving the construction of various different forms of apparatus, Ericsson has manufactured a solar boiler by which he obtains a pressure upon the piston of 35 pounds to the square inch and a velocity of 120 turns per minute. Such a motor may be favorably employed in hot countries. From the amount of heat which is developed in the apparatus the inventor deduces a solar temperature of 1,303,640°F., and he gives reasons for believing that this temperature is inferior to reality.—*La. Naturé*, April 5, 1884.

Hirn's Actinometer.—M. Hirn has invented an absolute actinometer, which is based on the following principle: A saturated vapor, contained in a closed recipient, takes a tension which corresponds to the lowest temperature of the enclosing walls. Imagine a still, the body of which is exposed to the sun, while the worm and the receiver are placed in the shade but in the open air. Place in the still a volatile liquid and make a vacuum in the apparatus, with the exception of the vapor which arises from the liquid. If the sky is completely covered the whole apparatus will be at the same temperature; the liquid will remain without distillation or condensation. As soon as the sky is uncovered and the sun's rays strike the still, the heat absorbed by the walls, instead of warming the liquid will make it boil, at the tension which corresponds to the minimum temperature of the apparatus and that of the refrigerant and the reservoir. If the surface of the latter is sufficient to disperse the heat continuously and rapidly in the surrounding air, the temperature of the whole apparatus will soon become stationary, and will be only slightly greater than that which will be indicated by a thermometer placed in the shade at the side of the refrigerator. The quantity of solar heat received in a unit of time will then be almost rigorously proportional to the quantity of liquid condensed in the unit of time. By the help of Regnault's equations the amount of solar heat absorbed by a known surface can then be regularly calculated, without resorting to any correction concerning accessory losses, the mass of liquid and of metal, etc. The best liquid for use seems to be bichloride of carbon.—*Chron. Industr.*, February 24, 1884. C.

Soap Roots.—According to the investigations of A. Rosoll, (Monatshefte für Chemie, 1884, p. 104), the saponin which is dissolved in the sap of the living roots of *Saponaria officinalis* and *Gypsophila struthium*, can be separated in the form of small, irregular white lumps, either by drying or by treating thin slices with absolute alcohol or ether.—*Dingler's Journal*, April 30, 1884.

ALUMINIUM AND ALUMINIUM BRONZE.—At a recent sitting of the Glasgow Physical Society, Prof. Jamieson communicated the result of his researches upon the electric qualities of aluminium. The metal was nearly pure, its density 2,786, its electric resistance 1·96 times that of pure copper-wire of the same length and diameter. For wires of equal length and weight the resistance of aluminium is a little less than that of pure copper. The addition of a small quantity of aluminium to copper largely increases both the mechanical and the electrical resistance. The first experiments furnished specimens in which the electric resistance was 25 times as great as that of pure copper. Such an alloy would be very useful in the manufacture of resistance coils. On account of its lightness pure aluminium wire might often be found desirable in military telegraphy.—*L'Electrician*, April 1, 1884.

PALMIERI'S ATMOSPHERIC ELECTRICITY.—Palmieri's Memoir on the Laws and Origin of Atmospheric Electricity, has been faithfully translated into German, by Heinrich Discher. The memoir is the result of 32 years observations made at the meteorological station of Mt. Vesuvious. After announcing the laws of atmospheric electricity, the illustrious professor describes the ingenious apparatus and the original methods of experimenting which he adopted in order to determine those laws, and which enabled him to observe the electrical state of the air under clear, cloudy, and rainy skies, and during volcanic eruptions. He attributes the atmospheric electricity to the accumulation of watery vapor in the air under the form of cloud, mist or rain.—*L'Electrician*, April 1, 1884.

NEW ELECTRO-MAGNET.—Sig. B. Ricco, of Palermo, rolls a long band of sheet iron around a nucleus of soft iron, insulating the different layers of the band by oiled paper. One pole is connected with the nucleus, to which the interior extremity of the band is soldered, and the other is connected with its exterior extremity. The current, in traversing the band, magnetises not only the nucleus but also each layer of the band, which thus plays the double role of conductor and of magnetic substance, thereby condensing the lines of force, and producing a great concentration of power.—*L'Electricité; Les Mondes*, March 8, 1884.

TANNING BY ELECTRICITY.—M. L. Gaulard suspends hides in a bath of tannin, which is traversed by an electric current. The hydrogen which is set free by the current acts upon the leather and destroys the nitrogenous matter. After eight days emersion in this bath, the tannin solution is replaced by another which is more concentrated, and the direction of the current is changed by reversing the poles of the electrodes. The oxygen then acts upon the liquid, oxidizing the tannin and precipitating it in the cells which are formed by the gelatine and fibrine of the hide.—*Chron. Industr.*, February 17, 1884.

GASES IN STEEL.—Brustlein concludes, from a great number of observations, that iron and steel can be alloyed with nascent hydrogen at ordinary temperatures; that at a red heat this alloy is broken up, but that at the melting temperature of steel it may again be alloyed with hydrogen with more or less energy.—*Soc. des Ing. Siv.*, April, 1883 C.

THE VOLCANIC ASHES OF KRAKATOA.—A. Renard has examined volcanic ashes which were gathered at Batavia, as well as some which were collected on board the German ship *Elizabeth*. They are finely powdered, of a greenish gray color, almost impalpable, and averaging about one-tenth of a millimetre in diameter. Under the microscope the dust seems to be mainly composed of vitreous fragments, riddled with bubbles. The particles of glass are often somewhat fibrous, more or less cylindrical and drawn out, as in certain forms of pumice. In some cases there is an irregular fracture showing curvilinear contours. The splinters are usually colorless, but sometimes they have a brownish tint. The results of the examination indicates a formation of cinders by the polarization of an igneous fluid mass, of which the particles are projected by the expansion of gases and submitted to a rapid cooling during their passage through the air.—*Bul. de l'Acad. de Belg.*, 1883. C.

PAPAL OBSERVATORY.—Pope Leo is not unmindful of intellectual and scientific progress. In the heart of the Lepini mountains a tall, fine tower has lately been erected which is intended for a meteorological observatory under the charge of Count Ludovico Pecci. Father Denza, director of the observatory of Moncalieri, under the orders and at the expense of Pope Leo, has bought all the instruments which are needed for such a station and pronounces everything admirably constructed and appropriate for the purpose designed. He thinks that the new observatory will be one of the most important in all Italy.—*Les Mondes*, March 29, 1884.

ORIGIN OF VOLCANIC ACTIVITY.—Few persons doubt, at the present day, that the elastic force of steam is the true motor of volcanic eruptions and earthquakes. The ocean is the source of the water which reascends to the surface through volcanic crevices. Since the pressure developed at great depths is the cause of the eruptions, it is obvious that the water can not penetrate through cavities of sensible dimensions. Stanislas Meunier supposes three successive layers of rocks under the ocean bed; the first of rocks impregnated with water, the second of rocks consolidated without the impregnation of water, and the third of rocks in which the temperature is sufficient both to vaporize and to dissociate the vapor of water. The water penetrates farther and farther into the deep rocks in consequence of the secular cooling of the globe, and the rupture of portions of the earth's crust by the contraction of the internal nucleus admits the water of the superficial layers into regions in which the vaporization and dissociation suddenly takes place.—*La. Nature*, May 10, 1884.

BALLOON PHOTOGRAPHY.—Major Eleslade has lately made some interesting experiments upon balloon photography for military purposes. He launched at Chatham captive balloons provided with automatic photographic chambers. After the balloon reached a certain height the plate was exposed and a negative taken. The experiments are said to have succeeded perfectly, and in one of the small proofs obtained in this way, the number of soldiers placed at a great distance could be ascertained by counting, with the aid of a magnifying glass, the little white spots which were made by the helmets of the infantry.—*La. Nature*, May 10, 1884.

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ON THE DEVELOPMENT OF THE THEORY OF THE STEAM ENGINE AND ITS APPLICATION. AN HISTORICAL OUTLINE SKETCH.

By ROBERT H. THURSTON.

[Prepared for the Montreal Meeting (1884) of the British Association for Advancement of Science.]

The following paper is intended to present, in the briefest possible form, an outline of the growth of the Theory of the Steam Engine, from its first and most primitive form to its most recent and most thoroughly practical development in application. It is not proposed to make this sketch in any sense complete, and it is hardly expected that it can be critically accurate. It may, however, prove interesting, and may be of real service, it is hoped, as presenting a distinct outline of what will, when more completely worked up, prove to be an exceedingly interesting and important detail of the history of applied science.

A complete history of the development of the Theory of the Steam Engine would include, first, the history of the Mechanical Theory of Heat; secondly, the history of the Science of Thermodynamics, which has been the outgrowth of that theory; third, the history of the application of the science of heat-transformation to the case of the Steam Engine; and, fourthly, an account of the completion of the Theory of the Steam and other Heat Engines by the introduction of the theory

of losses by the more or less avoidable forms of waste, as distinguished from those necessary and unavoidable wastes indicated by the pure theory of thermodynamics. The first and second of these divisions are treated of in works on thermodynamics, and in treatises on physics. The third division is briefly considered, and usually very incompletely, in treatises on the steam engine; while the last is of too recent development to be the the subject of complete treatment, as yet, in any existing works. The principal object of the present paper is simply to collect into a condensed form, and in proper relations, these several branches of the subject, leaving for another time and place that more full and complete account which might, did opportunity offer, be prepared to-day.

The "Mechanical Theory of Heat," as is now well understood, existed, as a speculation, from the days of the earliest philosophies. The contest which raged with such intensity, and sometimes acrimony, among speculative men of science, during the last century, was merely a repetition of struggles of which we find evidences, at intervals, throughout the whole period of recorded history. The closing period of this, which proved to be an important revolution in science, marked the beginning of the nineteenth century. It was inaugurated by the introduction of experimental investigation directed toward the crucial point of the question at issue. It terminated, about the middle of the century, with the acceptance of the general results of such experiment by every scientific man of acknowledged standing, on either side the Atlantic. The doctrine that heat was material, and its transfer a real movement of substance from the source to the receiver of heat, was thus finally completely superseded by the theory, now become an ascertained truth, that heat is a form of energy, and its transformation a change in the location and method of molecular vibration. The Dynamical Theory of Heat was first given a solid basis by the experiments of Count Rumford (Benjamin Thompson), in 1796–7—of which an account was given in a paper read by Rumford before the Royal Society of Great Britain in 1798—by the experiments of Sir Humphrey Davy in 1798–9, and by the later and more precise determinations of the value of the mechanical equivalent of heat, by Joule, in 1843, and subsequently.

The Science of Thermodynamics has for its essential basis the established fact of the dynamical nature of heat, and the fact of the quantivalence of two forms of energy—heat and mechanical motion, mole-

cular energy and mass energy. Resting, as it does, on fundamental, experimentally determined, principles, it could have no existence until, during the early part of the present century, these phenomena and these truths were well investigated and firmly established. Immediately upon the settlement of the controversy relating to the nature of heat, it became possible to commence the construction of the science which, asserting the mechanical theory of heat as as its fundamental fact, and the conservation and quantivalence of the two forms of energy as its fundamental principle, led to the determination of the method and extent of the transformation of the one into the other, during any prescribed series of physical changes.

It is not within the province of this paper to examine the claims made for rival philosophers, in the debate over the matter of priority of discovery of the mutual relations of the phenomena and principles of the new science. It is sufficiently evident that the revelation of the facts of the case led many minds to study the subject, and led to its nearly contemporaneous development in several countries. The first period of the development of the science was occupied almost exclusively by the exposition of the dynamical theory of heat, which lies at the bottom of the whole. This strikingly interesting and obviously important subject so absorbed the attention and occupied the thoughts of physicists that they seem hardly to have attempted to look beyond it, as a rule, and hence failed, at first, to see into what a magnificent department of theoretical and experimental investigation they were called. Mohr, in 1837; Seguin, in 1839; Mayer, of Heilbronn, in 1842, and Colding, in 1843, each took a step into a field, the limits of which and the importance of which they could at that time hardly have imagined. Mayer certainly had a very clear conception of the bearing of the new theory of heat upon dynamics, and exhibited remarkable insight into the far-reaching principles of the new science. He collated the facts more exactly determined later by Joule and others with the principle of the conservation of energy, and applied the rudiments of a science thus constructed to the calculation of the quantity of carbon and expenditure of heat which are unavoidably needed by a mountain climber, doing a given quantity of work, in the elevation of his own body to a specified height. The work of Mayer may be taken as representing the first step in the production of a Science of Thermodynamics, and in the deduction of the consequences of the fact which had, until his time, so seldom engaged the attention of

men of science. It was only about the middle of the century that it began to be plainly seen that there existed such a science, and that the dynamic equivalence of heat, and energy in the mechanical form, was but a single fact, which must be taken in connection with the general principles of the persistence of energy, and applied in all cases of performance of work by expenditure of heat through the action of elastic bodies.

The Development of the Science of Thermodynamics into available and satisfactory form was effected mainly by Professors Rankine and Clausius, working independently but contemporaneously from 1849. Clausius developed the general theory with beautiful clearness and conciseness of mathematical method and work, and succeeded in constructing a complete system, almost equal in extent and exactness to the geometrical system of Euclid. Rankine, producing the same results, in part, by his wonderfully condensed method of treatment, turned his attention more closely to the application of the theory to the case of the steam and other heat engines, giving, finally, in his "Prime Movers" (1859), a concise yet full exposition of the correct theory of those motors, so far as it is possible to do so by purely thermodynamic treatment. He was unaware, apparently, as were all the scientific men of his time, of the extent to which the conclusions reached by such treatment of the case are modified, in real engines, by the interference of other physical principles than those taken cognizance of by his science. Sir Wm. Thompson, partly independently, and partly working with Joule, has added much valuable work to that done by Clausius and Rankine. In the hands of these great men the science took form, and has now assumed its place among the most important of all branches of physical science.

The Theory of the Steam Engine, like every other scientific system, rests upon a foundation of facts ascertained by experiment, and of principles determined by the careful study of the laws relating to those facts, and controlling phenomena, properly classed together by that science. Like every other element entering into the composition of a scientific system, this theory has been developed subsequently to the establishment of its fundamental facts, and the history of progress in the art to which it relates shows that the art has led the science from the first. The theory of the steam engine includes all the phenomena and all the principles involved in the production of power, by means of the steam engine, from the heat-energy derived from the chemical

combination of a combustible with the oxygen of the air acting as a supporter of the combustion. The complete theory therefore includes the theory of combustion; the consideration of the methods of development and transfer, and of losses of heat in the steam boiler; the examination of the methods of transfer of heat-energy from boiler to engine, and of waste of heat in this transfer, and, finally, the development of mechanical energy in the engine, and its application, beyond the engine, to the machinery of transmission, with an investigation of the nature and method of waste in this last transformation. It is, however, only the last of these divisions of the subject that it is here proposed to consider. The remaining portion of this paper will be devoted to the tracing of the growth of the theory of the steam engine, simply as a mechanical instrument for transformation of the one form of energy into the other—of the molecular energy of heat motion, as stored in the vapor of water, into mass energy, mechanical energy, as applied to the driving of mechanism. The theory thus limited includes a study of the thermodynamic phenomena, as the principal and essential operations involved in the performance of work by the engine; it further includes the consideration of the other physical processes which attend this main function of the engine, and which, inevitably and unavoidably, so far as is to-day known, concur in the production of a waste of energy.

Of all the heat sent forward by the steam boiler to the engine, a certain part, definite in amount and easily calculated when the power developed is known, is expended by transformation into mechanical energy; another part, equally definite and easily calculated, also, is expended as the necessarily occurring waste which must take place in all such transformations, at usual temperatures of reception and rejection of heat; still another portion is lost by conduction and radiation to surrounding bodies; and, finally, a part, often very large in comparison with even the first and principal of these quantities, is wasted by transfer, within the engine, from the induction to the eduction side, "from steam to exhaust," by a singular and interesting process, without conversion into useful effect. The science of thermodynamics only takes cognizance of the first, which is sometimes one of the smallest of these expenditures. The science of the general physics of heat takes cognizance of the others.

The Science of the Phenomena of the Steam Engine must, like every other branch of applied science, be considered as the product of

two distinct processes of development; the one is what may be called the experimental development of the subject, the other is the purely theoretical progress of the science. So far as the useful application of principles to the perfection of the machine is concerned, the latter has always, as is usually the case elsewhere, been in advance of the former in its deduction of general principles; while, as invariably, the former has kept far in advance, in the working out of practically useful results, and in the determination of the exact facts where questions of economic importance have arisen. It is proposed here to follow the history of the experimental development of the principles controlling the efficiency of the engine, and modifying the conclusions derived by the application of the science of heat transformation, after first tracing the progress of the development of that science. The gradual formation of the pure theory of the steam engine will be traced, and the limitations of that theory will naturally come up for consideration afterward.

The germ of a Science of the Steam Engine may be found in the work of Sadi Carnot, published just sixty years ago. Although familiar with the then doubted mechanical theory of heat, he was not sufficiently well convinced of its correctness, apparently, to make it the basis of his work, but assumed, throughout his "*Reflexions sur la Puissance Motrice du Feu*," the theory of substantial caloric. Nevertheless, in his development of the theory of heat engines, he enunciated some essential principles, and thus laid the foundation for a theory of the steam engine which was given correct form, in all its details, as soon as the dynamical theory was taken for its foundation principle. Carnot asserts that "The motive power of heat is independent of the means taken to develop it; its amount is determined, simply, by the temperature of the bodies between which the heat is transferred. Wherever there exists a difference of temperature, there may be a development of power. The maximum amount of power obtainable by the use of steam is the maximum obtainable by any means whatever. High pressure engines derive their advantage over low pressure engines simply from their power of making useful a greater range of temperature." He made use of the device known as the "Carnot Cycle," exhibiting the successive expansions and compressions of the working fluid in heat engines, in the process of change of volume and temperature, while following the series of changes which gives the means of transformation of heat into power with final restoration of the fluid to its initial condition, showing that such a complete cycle must be tra-

versed in order to determine what proportion of the heat energy available can be utilized by conversion into mechanical energy. This is one of the most essential of all the principles comprehended in the modern science. This "Carnot Cycle" was, afterward, represented graphically by Clapeyron.

Carnot shows that the maximum possible efficiency of fluid is attained, in heat engines, by expanding the working fluid from the maximum attainable temperature and pressure down to the minimum temperature and pressure that can be permanently maintained on the side of condensation or rejection, *i. e.*, if we assume expansion according to the hyperbolic law, by adopting, as the ratio of expansion, the quotient of maximum pressure divided by back pressure. He further shows that the expansion, to give maximum efficiency, should be perfectly adiabatic. These principles have been recognized as correct by all authorities, from the time of Carnot to the present time, and have been, not infrequently, brought forward as new by minor later writers unfamiliar with the literature of the subject. Introducing into the work of Carnot the dynamical relation of heat and work, a relation, as shown by other writings, well understood, if not advocated publicly by him, the theory of the steam engine becomes well defined and substantially accurate. The Count de Pambour, writing in 1835, and later, takes up the problem of maximum efficiency of the steam engine, shows the distinction to be drawn between the efficiency of fluid and efficiency of machine, and determines the value of the ratio of expansion for maximum efficiency of engine. He makes this ratio equal to the quotient of maximum initial pressure divided by the sum of the useless internal resistances of the engine, including back pressure and friction, and reduced to equivalent pressure per unit of area of piston. This result has been generally accepted, although sometimes questioned, and has been demonstrated anew, in apparent ignorance of the fact of its prior publication by De Pambour, by more than one later writer. De Pambour, applying his methods to the locomotive, particularly, solved the problem, since distinctively known by his name: Given the quantity of steam furnished by the boiler in the unit of time, and the measure of resistance to the motion of the engine; to determine the speed attainable.

Professor Thomas Tate, writing his "Mechanical Philosophy," in 1853, gives the principle stated above a broader enunciation, thus: "The pressure of the steam, at the end of the stroke, is equal to the

sum of the resistances of the unloaded engine, whatever may be the law expressing the relation of volume and pressure of steam."

Professor Clausius, as has been already stated, applied the modern theory of the steam engine to the solution of the various problems which arise in the practice of the engineer, so far as they can be solved by the principles of thermodynamics. His papers on this subject were printed in 1856. The Count de Pambour had taken a purely mechanical mode of treatment, basing his calculations of the work done in the cylinder of the steam engine upon the hypothesis of Watt, that the weight of steam acting in the engine remained constant during expansion, and that the same assumption was applicable to the expanding mass contained in engine and boiler during the period of admission. He had constructed empirical formulas, published in his work on the theory of the steam engine, in 1844, for the relation of volume and pressure, during expansion, and had based his determinations of the quantity of work done, and of expenditure of steam in the engine, upon this set of assumptions and formulas, considering the steam to remain in its initial condition of dry and saturated vapor, or of moist vapor, as the case may be, from the beginning to the end of the stroke. Errors were thus introduced, which, although not important in comparison with those often occurring when the results of purely thermodynamic, and in so far correct, treatment was compared with the actual case, were, nevertheless, sufficiently great to become noticeable when the true theory of heat engines became known, and correctly applied. Clausius proved that, in the expansion of dry and saturated steam, doing work in the engine, condensation must take place to a certain extent, and that, consequently, the weight of steam in the cylinder must be somewhat reduced by the process of expansion beyond the point of "cut-off." During the period of compression, also, the reverse effect must occur, and the compressed mass must become superheated, if initially dry. He showed that the amount of work actually done in a non-conducting working cylinder must be sensibly different from that estimated by the method of De Pambour. Taking advantage of the redetermination of the constants in Regnault's equations effected by Moritz. Clausius obtains numerical results in the application of the true theory, and deduces the amount of work done in the steam engine under various conditions such as are met with in practice. He shows how the action of the engine may be made that of the Carnot Cycle, and determines the effect of variation of the temperature of the "prime"

steam. The investigation is, in the main, purely theoretical, no application is made to the cases met with in real work, and the comparison of the results of the application of the new theory to practice in steam engineering is left for others.

The work of Clausius is, throughout, perfectly logical, and beautifully simple and concise, and his application of the theory to the steam engine amounts to a complete reconstruction of the work of Carnot, and his followers, upon a correct basis. He developes with mathematical exactness of method and work the fundamental principles of the science of thermodynamics, constructs the "fundamental equations," the so-called "General Equations of Thermodynamics," and, in the course of his work, proves the fact of the partial condensation of saturated steam, when permitted to expand doing work against resistance.

Professor Rankine began his work upon the theory of the transformation of heat into mechanical energy at about the same time with Clausius (1849), and published his first important deduction, the form of the General Equation of Thermodynamics, nearly simultaneously, but a little earlier. He gave much attention to the then incomplete work of development of applied thermodynamics, and produced not only the whole theory of the science, but very extended papers, including solutions of practical problems in the application of the science to heat engines. Stating with singular brevity and clearness the main principles, and developing the general equations in substantially the same form, but by less easily followed processes, than his contemporary, he proceeded at once to their application. He determines the thermodynamic functions for air and other gases, exhibits the theory of the hot-air engine, as applied to the more important and typical forms, deduces expressions for their efficiency, and estimates the amount of heat demanded, and of fuel consumed, in their operation, assuming no other expenditure of heat than that required in an engine free from losses by conduction and radiation. He next, in a similar manner, applies the theory to the steam engine, proves the fact of the condensation of steam during the period of expansion, estimates the amount of heat, fuel, and steam expended, and the quantity of work done, and determines thus the efficiency of the engine. He makes a special case of the engine using superheated steam, as well as that of the "jacketted" engine, considers the superheated steam engine, and the binary vapor engine, and reconstructs De Pambour's problem. Applying the theory

of the steam engine to a considerable number of cases, differing in the steam pressure and in the ratio of expansion adopted, and including both condensing and non-condensing engines, he constructs a table exhibiting the efficiency of the steam, and the probable consumption of fuel (assuming a somewhat low efficiency of boiler) which table represents the limit of efficiency under the assumed conditions, a limit which may be approached as the conditions of practice approximate to those of the ideal cases taken, but which can never be reached.

As Rankine was not aware of the often enormous difference produced in the performance of the steam engine by the extra thermodynamic phenomena involved in its operation, he does not indicate the fact that the results of his calculations must be taken with the qualification just stated above, and his figures are still sometimes supposed to represent those of actual performance. The fact is, however, that the consumption of steam, and of fuel, in actual practice, always considerably exceeds those obtained by the solution of the thermodynamic problem, and, often, as already stated, exceeds that quantity by a very large amount.

Since the time of Rankine's and Clausius' investigations, the thermodynamic theory of the steam engine has received no important modifications, and the work of later engineers, and of physicists working upon the general subject, has been confined to the study, experimental or other, of the limitations set to the application of this theory by the influence of other physical phenomena.

Rankine's work included the construction of a remarkably exact, though hypothetical, equation expressing the relation of temperatures and pressures of vapors, based upon his theory of "molecular vortices," a comparison of the efficiencies of air and steam engines working between the same limits of temperature, and an exceedingly beautiful method of graphically determining the most economical size of steam engine, from the commercial point of view, the quantity of power required being given, and all expenses being calculable. He defined and outlined the science of "energetics," established the beginnings of a system of graphical thermodynamics, including the representation of the action of steam in the compound engine. He studied the action of explosive gas engines, and calculated the explosive energy of liquids heated under pressure. Besides all this, Rankine performed an enormous amount of work in mathematical physics, in hydrodynamics, in hydromechanics, in the theory of naval architecture, and

in the application of mechanics to general engineering. Several important text-books, a large volume on shipbuilding, and other works, with an unknown number of papers, published and unpublished, form a monument to the power and industry of this wonderful man and remarkable genius, that may be looked upon as perhaps the greatest wonder of the intellectual world.

The Thermodynamic Theory of the Steam Engine stands, to-day, substantially as it was left by Clausius and Rankine at the close of their work in this field, in the decade 1850 to 1860. Many treatises have been published, some of them by men of exceptional ability; but all have followed the general line first drawn by these masters, and have only now and then found some minor point to develop. Rankine's "Steam Engine and other Prime Movers," written a quarter of a century ago, is still a standard work on the theory of the heat engines, and is still used as a text-book in engineering schools in this country and Europe.

The Limitations of the Thermodynamic Theory of the Heat Engines, and of its application in the design and operation of such engines were first discovered by James Watt, a hundred years ago and more. They were systematically and experimentally investigated by Isherwood, in 1855 to 1865, were observed and correctly interpreted by Clark, in 1855 and earlier, and were revealed again by the experiments of Hirn, and by those of Emery and many other recent investigators on both sides of the Atlantic. These limitations are due to the fact that losses occur in the operation of steam engines which are not taken into account by the hitherto accepted theory of the engine, and have no place in the thermodynamic treatment of the case.

It is usually assumed, in the usual theory of the engine, that the expansion of the working fluid takes place in a cylinder having walls impermeable to heat, and in which no losses by conduction or radiation, or by leakage, can occur. Of those losses which actually take place in the real engine, that due to leakage may be prevented, or, if occurring, can be checked; but it is impossible, so far as is now known, to secure a working cylinder of perfectly non-conducting material. The consequence is that, since the steam or other working fluid enters at a high temperature and is discharged at a comparatively low temperature, the surfaces of cylinder, cylinder heads and piston, are, at one instant, charged with heat of high temperature, and at the next moment, exposed to lower temperatures, are drained of their surplus

heat, which heat is then rejected from the cylinder and wasted. Thus, at each stroke, the metal surfaces, exposed to the action of the expanding substance, alternately absorb heat from it, and surrender that heat to the "exhaust." In the case of the gas engines, this waste is rendered enormously greater by the action of the water-jacket, which is there needed to keep the cylinder down to a safe temperature, and which takes away, in the circulating stream of cooling water, an immense amount—usually about one half—of the heat received from the burning gas. In the steam engine, the loss by the method here referred to is rarely less than one-fourth, in unjacketted cylinders, and is often more than equal to the whole quantity of heat transformed into mechanical energy. The amount of this loss increases with wet steam and is diminished by any expedient, as steam-jacketting or superheating, which prevents the introduction or the production of moisture in the midst of the mass of steam in the cylinder. As the range of temperature worked through in the engine increases, as the quantity of steam worked per stroke diminishes, and as the time allowed for transfer of heat to and from the sides and ends of the cylinder and the piston is increased, the magnitude of this loss increases. Hence the use of high steam, of a high ratio of expansion, and of low piston speed, tends to increase the amount of this waste, while low steam, a low ratio of expansion and high engine speed, tend to diminish it. These physical phenomena are therefore no less important in their influence upon the behavior of the engine, and upon its efficiency, and are no less essential elements for consideration in the general theory of the engine than those taken into account in the purely thermodynamic theory.

James Watt, as above stated, discovered this cause of the limitation of the efficiency of the steam engine. He not only discovered the fact of the existence of this method of waste, but experimentally determined its amount in the first engine ever placed in his hands. It was in 1763 that he was called upon to repair the little model of the Newcomen engine, then and still in the cabinets of the University of Glasgow. Making a new boiler, he set up the machine and began his experiments. He found, to his surprise, that the little steam cylinder demanded four times its own volume, at every stroke, thus wasting, as he says, three-fourths of the steam supplied, and requiring four times as much "injection-water" as should suffice to condense a cylinder full of steam. It was in the course of this investigation that he discovered

the existence of so-called "latent heat." All of Watt's first inventions were directed toward the reduction of this immense waste. He proposed to himself the problem of keeping the cylinder "as hot as the steam that entered it;" he solved this problem by the invention of the separate condenser and the steam jacket, and thus the discovery of the limitation of the thermodynamic theory here noted was the source of Watt's fame and fortune.

John Smeaton, a distinguished contemporary of Watt, and perhaps the most distinguished engineer of his time, seems to have been not only well aware of this defect of the steam engine, but was possibly even in advance of Watt in attempting to remedy it. He built a large number of Newcomen engines between 1765 and 1770, in some, if not many of which, he attempted to check loss of this now familiar "cylinder condensation" in engines, some of which were five and six feet in diameter of cylinder, by lining pistons and heads with wood. This practice may not be practicable with the temperatures now usual; but no attempt has been made, so far as is known to the writer, to follow Smeaton in his thoroughly philosophical plan of improvement. Cylinder-condensation remains to-day, as in the time of Smeaton and Watt, the chief source of waste in all well designed and well constructed heat engines.

It is a curious fact, and one of great interest as illustrating the gulf formerly separating the philosopher, studying the steam engine and working out its theory, from the practitioner engaged in its construction and operation, in the earlier days of engineering, that, notwithstanding the fact that this waste was familiar to all intelligent engineers, from the time of the invention of the modern steam engine, and was recorded in all treatises on engine construction and management, the writers on the theory of the machine have apparently never been aware that it gives rise to the production, in the working cylinder, of a large amount of water mingled with the steam. In fact it has often been assumed by engineers themselves, that this water is always due to "priming" at the boiler. Even Rankine, writing in 1849–'50, while correctly describing the phenomenon of cylinder-condensation, made the mistake of attributing the presence of the water in steam cylinders to the fact of condensation of dry steam doing work by expansion, apparently not having noted the fact that this would only account for a very insignificant proportion of the moisture actually present in the average

steam engine. He considers incomplete expansion the principal source of loss, as do usually other writers on thermodynamics.

Thomas Tredgold, writing in 1827, who, but little later than Carnot, puts the limit to economical expansion at the point subsequently indicated and more fully demonstrated by De Pambour, exaggerates the losses due to the practical conditions, but evidently does perceive their nature and general effect. He also shows that under the conditions assumed, the losses may be reduced to a minimum, so far as being dependent upon the form of the cylinder, by making the stroke twice the diameter.

The limit of efficiency in heat engines, as has been seen, is thermodynamically determined by the limit of complete expansion. So well is this understood, and so generally is this assumed to represent the practical limit, by writers unfamiliar with the operation of the steam engine, that every treatise on the subject is largely devoted to the examination of the amount of the loss due to what is always known as "incomplete expansion"—expansion terminating at a pressure higher than the back pressure in the cylinder. The causes of the practical limitation of the ratio of expansion to a very much lower value than those which maximum efficiency of fluid would seem to demand, have not been usually considered, either with care or with intelligence, by writers thoroughly familiar with the dynamical treatment, apart from the modifying conditions here under consideration.

Watt, and probably his contemporaries and successors, for many years supposed that the irregularity of motion due to the variable pressure occurring with high expansion was the limiting condition, and does not at first seem to have realized that the cylinder-condensation discovered by him had any economical bearing upon the ratio of expansion at maximum efficiency. It undoubtedly is the fact that this irregularity was the first limiting condition with the large, cumbrous, long-stroked, and slow-moving engines of his time. Every accepted authority from that day to the present, has assumed, tacitly, that this method of waste has no influence upon the value of that ratio, if we except one or two writers who were practitioners rather than scientific authorities.

Mr. D. K. Clark, publishing his "Railway Machinery," in 1855, was the first to discuss this subject with knowledge, and with a clear understanding of the effects of condensation in the cylinder of the steam engine, upon its maximum efficiency. Cornish engines, from

the beginning, had been restricted in their ratio of expansion to about one-fourth, as a maximum, Watt himself adopting a "cut-off" at from one-half to two-thirds. Hornblower, with his compound engine competing with the single cylinder engines of Watt, had struck upon this rock, and had been beaten in economy by the latter, although using much greater ratios of expansion; but Clark, a half centuıy and more later, was, nevertheless, the first to perceive precisely where the obstacle lay, and to state explicitly that the fact that increasing expansion leads to increasing losses by cylinder condensation, the losses increasing in a much higher ratio than the gain, is the practical obstruction in our progress toward greater economy.

Clark, after a long and arduous series of trials of locomotive engines, and prolonged experiment looking to the measurement of the magnitude of the waste produced as above described, concludes: "The magnitude of the loss is so great as to defeat all such attempts at economy of fuel and steam by expansive working, and it affords a sufficient explanation of the fact, in engineering practice, that expansive working has been found to be expensive working, and that, in many cases, an absolutely greater quantity of fuel has been consumed in extended expansion working, while less power has been developed." He states that high speed reduces the effect of this cause of loss, and indicates other methods of checking it. He states that "the less the period of admission, relative to the whole stroke, the greater the quantity of free water existing in the cylinder." His experiments, revealing these facts were, in some cases, made prior to 1852. But the men handling the engines had observed this effect even before Clark; he states that they rarely voluntarily adopted "a suppression of above 30 per cent.," as they found the loss by condensation greater than the gain by expansion. Describing the method of this loss, this author goes on to say that "to prevent entirely the condensation of steam worked expansively, the cylinder must not only be simply protected by the non-conductor; it must be maintained, by independent external means, at the initial temperature of the steam." He thus reiterates the principle expressed by Watt, three-quarters of a century before, and applies it to the newly stated case.

The same author, writing in 1877, says: "The only obstacle to the working of steam advantageously to a high degree of expansion in one cylinder, in general practice, is the condensation to which it is subjected, when it is admitted into the cylinder at the beginning of the

stroke, by the less hot surfaces of the cylinder and piston; the proportion of which is increased so that the economy of steam by expansive working ceases to increase when the period of admission is reduced down to a certain fraction of the stroke, and that, on the contrary, the efficiency of the steam is diminished as the period of admission is reduced below that fraction." The magnitude of this influence may be understood from the fact that the distinguished engineer, Loftus Perkins, using steam of 300 pounds pressure, and attaining the highest economy known, up to his time, found his engine to consume 1·62 pounds of fuel per hour and per horse-power; while this figure is now reached by engines using steam at one-third that pressure, and expanding about the same amount, and sometimes less.

Mr. Humphreys, writing a little later than Clark, shows the consumption of fuel to increase seriously as the ratio of expansion is increased beyond the very low figure which constituted the limit in marine engines of his time.

Mr. B. F. Isherwood, a Chief Engineer in the United States Navy, and later Chief of the Bureau of Steam Engineering, seems to have been the first to have attempted to determine, by systematic experiment, the law of variation of the amount of cylinder-condensation with variation of the ratio of expansion, in unjacketed cylinders. Experimenting on board the U. S. S. *Michigan*, he found that the consumption of fuel and of steam was greater when the expansion was carried beyond about one-half stroke than when restricted to lower ratios. He determined the quantity of steam used, and the amount condensed, at expansions ranging from full stroke to a cut-off at one-tenth. His results permit the determination of the method of variation, with practically satisfactory accuracy, for the engine upon which the investigation was made, and for others of its class. It was the first of a number of such investigations made by the same hand, and these to-day constitute the principal part of our data in this direction. The writer, studying these results, found that the cylinder condensation varied sensibly as the square root of the ratio of expansion, and this is apparently true for other forms and proportions of engine. The amount of such condensation usually lies between one-tenth and one-fifth the square root of that ratio, if estimated as a fraction of the quantity of steam demanded by a similar engine having a non-conducting cylinder.

The state of the prevalent opinion on this subject, at the time of this

work of Clark and of Isherwood, is well expressed by the distinguished German engineer, Dr. Albans, who, writing about 1840, says of the choice of best ratio of expansion: "Practical considerations form the best guide, and these are often left entirely out of view by mathematicians. Many theoretical calculations have been made to determine the point, but they appear contradictory and unsatisfactory." Renwick, in 1848, makes the ratio of initial divided by back pressure the proper ratio of expansion, but correctly describes the effect of the steam jacket, and suggests that it may have peculiar value in expansive working, and that the steam may receive heat from a cylinder thus kept at the temperature of the "prime" steam. John Bourne, the earliest of now acknowledged authorities on the management and construction of the steam engine, pointed out, at a very early date, the fact of a restricted economic expansion. Rankine recognized no such restriction as is here under consideration, considered the ratio of expansion at maximum efficiency to be the same as that stated by Carnot, and by other early writers, and only perceived its limitation by commercial considerations, a method of limitation of great importance, but often of less practical effect than is the waste by condensation. In his life of Elder (1871), however, he indicates the existence of a limit in practice, and places the figure at that previously given by Isherwood, for unjacketted engines. By this latter date, the subject had become so familiar to engineers that a writer in "London Engineering," in 1874, contemns writers who had neglected to observe this limitation of efficiency as indulging in "mediæval twaddle."

A few writers on thermodynamics finally came to understand the fact that such a limitation of applied theory existed. Mons. G. A. Hirn, who, better than probably any authority of his time or earlier, combined a knowledge of the scientific principles involved, with practical experience and experimental knowledge, in his treatise on thermodynamics (1876), concludes: "*qu'il est absolument impossible d'édifier* a priori *une théorie de la machine á vapeur d' eau douce d'un charactere scientifique et exact,*" in consequence of the operation of the causes here detailed. While working up his experiments upon the performance of engines, comparing the volume of steam used with that of the cylinder, he had always found a great excess, and had, at first, attributed it to the leakage of steam past the piston; but a suggestion of M. Leloutre set him upon the right track, and he came to the same conclusion as had Watt, so many years before. He explains that errors of thirty, or even

up to seventy, per cent., may arise from the neglect of the consideration of this loss. Combes had perceived the importance of this matter, and De Freminville suggested the now familiar expedient of compression, on the return stroke, as nearly as possible to boiler pressure, as a good way to correct the evil. The matter is now well understood by contemporary writers, and it has become fully agreed, among theoretical writers as well as among practitioners, that the benefit of extended expansion in real engines can only be approximated to that predicted, by the theory of the ideal engine, by special arrangements having for their object the reduction of cylinder waste, such as superheating, "steam jacketing" and "compounding."

Professor Cotterill has given more attention to this subject than any writer up to the present time. He devotes a considerable amount of space to the study of the method of absorption and surrender of heat by the metal surfaces enclosing the steam, constructs diagrams which beautifully illustrate this action, and solves the problems studied by him with equal precision and elegance of method. He summarizes the experimental work done to the date of writing, and very fully and clearly exhibits the mode of transfer of heat past the piston without transformation into work. Professor Cotterill's treatise on the steam, "considered as a heat engine," is invaluable to the engineer.

Thus the theory of the steam engine stands to-day, incomplete, but on the verge of completion, needing only a little well directed experimental work to supply the doubtful elements. Even these are becoming determined. Isherwood gives facts showing waste to be proportional, very nearly, if not exactly, to the square root of the ratio of expansion, and Escher, of Zurich, has shown the loss to be also proportional to the square root of the time of exposure, or, in other words, to the reciprocal of the square root of the speed of rotation, and it only remains to determine the method of variation of loss with variation of range of temperature to give the whole of the necessary material for the construction of a working theory which will enable the engineer to estimate, in advance of construction, the economic performance of his machine. There will, undoubtedly be much more to be done in constructing an exact theory involving all the physical changes occurring in the working of the heat engines familiar to us; but it will yet be done, and probably very soon. It is the hope of the writer that experiments made under his direction, recently, may furnish the needed data, as the result of the first systematic research directed

to that end; but if this should prove not to be the fact, it cannot be long before direct investigation will secure all essential knowledge. When this is the case the remarks of those distinguished physicists and engineers, Hallauer, and his great teacher, Hirn, will be no longer well based upon apparent fact.

Says Hirn, in his memorable discussion with Zeuner, in regard to this subject, "*Ma conviction reste aujourd'hui qu'elle etait il y a vingt ans, une théorie proprement dite de la machine à vapeur est impossible; la théorie experimentale, etablie sur le moteur luimeme et dans toutes les formes ou il a été essayé, en mecanique appliqué peut seule conduire a des resultats rigoureux.*"

At present, it seems only possible, in the absence of a complete experimental examination, to do more than to base the determination of the ratio of maximum efficiency upon such experience as is familar to engineers. Mr. C. E. Emery considers that, for common unjacketed engines, it is practically safe to take the ratio for maximum duty at a figure expressed by an empirical formula proposed by him: $r = (p + 37) \div 22$. The writer has usually taken it, in estimates, as not far from one-half the square root of the boiler pressure expressed, as before, in pounds on the square inch. These points of cut-off are reduced still further by the fact that, commercially, it is better to reduce the size of engine at the expense of efficiency, as the cost of fuel and of similarly variable expenses increase. This is however, a matter for the treatment of which space cannot here be taken. Rankine has devised a convenient method of solving such problems, involving this condition, as may arise in practice, where cylinder-condensation may be neglected, and the writer has found a method of adapting it to ordinary practice. The subject will ultimately form, properly, a final division of the complete theory of the steam engine.

Chronologically considered it is seen that the history of the growth of the theory of the steam engine divides itself distinctly into three periods, the first extending up to the middle of the present century, and mainly distinguished by the attempts of Carnot and of Clapeyron to formulate a physical theory of the thermodynamics of the machine, the second beginning with the date of the work of Rankine and Clausius, who constructed a correct thermodynamic theory, and the third beginning a generation later, and marked by the introduction, into the general theory, of the physics of the conduction and transfer of that heat which plays no part in the useful transformation of energy. The first period

may be said to include, also, the inauguration of experimental investigation, and the discovery of the nature and extent of avoidable wastes and attempts at their amelioration by James Watt and by John Smeaton. The second period is marked by the attempt, on the part of a number of engineers, to determine the method and magnitude of these wastes by more thorough and systematic investigation, and the exact enunciation of the law governing the necessary rejection of heat, as revealed by the science of thermodynamics. The third period is opening with promise of a complete and practically applicable investigation of all the methods of loss of energy in the engine, and of the determination, by both theoretical and experimental research, of all the data needed for the construction of a working theory.

Mons. Hirn has recognized these three periods, and has proposed to call the second "theoretical" and the third the "experimental" stage. The writer would prefer to make the nomenclature somewhat more accordant with what has seemed to him to be the true method of development of the subject. It has been seen that the experimental stage really began with the investigations of Watt in the first period, and that the work of experimentation was continued through the second into the present, the last period.

It is also evident that the theoretical stage, if it can be properly said that such a period may be marked off in the history of the theory of the steam engine, actually extends into the present epoch; since the work of the engineer and the physicist of to-day consists in the application of the science of heat-transfer and heat-transformation, together, to the engine. During the second period the theory included only the thermodynamics of the engine; while the third period is about to incorporate the theory of conduction and radiation into the general theory with the already established theory of heat-transformation. The writer would therefore make the classification of these successive stages in the progress here described thus:

1. Primary Period.—That of incomplete investigation and of earliest systematic, but inaccurate theory.

2. Secondary Period.—That of the establishment of a correct thermodynamic theory, the *Theory of the Ideal Engine.*

3. Tertiary Period.—That of the production of the complete theory of the engine, of the *true Theory of the Real Engine.*

The work of developing this theory is still incomplete. It remains to be determined, by experiment, precisely what are the laws of transfer

of heat between metal and vapor, in the engine cylinder, and to apply these laws in the theory of the machine. Cotterill has shown how heat penetrates and traverses the metal and Grashof has indicated the existence of an intermediate and approximately constant, temperature, between the temperatures of the initial steam and of the exhaust, and both have given us some new methods. The writer, while pointing out the nature of the true "curve of efficiency" of the steam engine which he was so fortunate as to discover, has shown how it may be made useful in the solution of practical and of theoretical problems involved in the applied theory of heat engines and many able minds are now engaged upon the theory. There can be little doubt that it will soon become satisfactorily complete.

HOBOKEN, N. J., July, 1884.

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Changes of Refrangibility in Electric Spectra.—It is well known that the hydrogen lines, in the spectra of solar spots and protuberances, are often suddenly displaced, so as to appear wavy or broken. This is especially the case with the C line. The velocities impressed upon the gaseous particles must be so enormous that there seemed little likelihood of reproducing the phenomena in the labratory. Cazin supposed that electricity might have sufficient velocity, but he felt the need of new investigations in that direction. Liveing and Dewar found that when a few drops of water were injected into the voltaic arc, there was a sudden enlargement of the hydrogen lines, quite comparable to that which is often observed in the solar atmosphere and attributed to eruptions. Fievey found similar phenomena, by passing a series of electric sparks between electrodes of magnesium which were only one or two millimetres apart, in a tube of three centimetres diameter, which was filled with hydrogen under a mercurial pressure of two metres. He employed a Christie spectroscope which gave, with a half prism, a dispersion equivalent to that of six flint prisms. The spark was furnished by a large coil, connected with a condenser and driven by a battery of ten bi-chromate of potassium elements. The electrodes were perpendicular to the slit, which was so regulated as to give the sodium lines with sharp definition. An objective was interposed between the electrodes and the slit. Under these circumstances the C hydrogen line and the magnesium lines b_1, b_2 and b_3 undulated, expanded and broke on the right and on the left.—*Bull. de l'Acad. Belg.* 1884. C.

Sunsets in China.—Dechevrens, the director of the Observatory at Zi-kæ-wei, reports that the brilliant twilights and the green sun were observed in China, as well as in Europe, during the last autumn. On the other hand, during the winter, the zodiacal light appeared to him fainter than on preceding years. This corresponds with the testimony of Thollon and Perrotin at Nice, and of Janssen, at Meudon.—*Les Mondes*, April 18, 1884. C.

OUR CLOTHING AND OUR HOUSES.

By Louis W. Atlee, M. D.

All the English writers on the subject of Hygiene, including even Parkes and Wilson, whose systematic treatises are excellent in many respects, are signally deficient on the subjects of clothing and of houses. Of the truth of this any one can satisfy himself very speedily by trying to collect information about them; he will be forced to consult writers in other languages for what he wishes to know.

Necessary to our health and comfort as a knowledge of clothing and dwellings must be, it may be said, without exaggeration that more attention is devoted in our medical publications to new pharmaceutical preparations, and to ingenious pathological and etiological theories than to what is of such every-day practical intent.

In ordinary works on Hygiene the subject of sewerage is surely sufficiently well discussed and this may be true, also, for water supply. On the subject of clothing and of houses what is about to be said is almost entirely new to the English language. It is mainly in French writings and above all in an article in "La Révue des deux Mondes," for July 15, 1883, that the following ideas were found.

Our clothes and houses are the armor we use to protect us in our incessant fight with the elements. They are not destined to isolate us from the surrounding air, but to regulate our incessant and indispensable relations with it. These relations cannot be clearly understood without a knowledge of how the body is maintained at so equable a temperature under the most diverse influences. We know that the animal heat is produced by the chemical metamorphoses that take place in the tissues and principally, but not exclusively, by the combustion of the assimilated food carried into the circulation, which the oxygen respired, changes into carbonic acid and water. These combustions elevate the temperature of the blood, and the warm liquid that penetrates everywhere, heats the organism almost in the same way that a water-furnace heats a house. The activity of the respiration and the consumption of oxygen decreases during sleep; it is increased on the contrary during exercise, a part of the heat being converted into mechanical work. We may admit that a man who takes but little exercise, respires in the 24 hours, 10 c. m. of air, and absorbs about a quarter of the oxygen contained in it, say 650 grams of oxygen. The heat liberated by

these chemical actions may reach at a medium about 2,000 or 3,000 heat units; it would be sufficient to bring 20 or 30 litres of water to the boiling point, or to make the temperature of the body go up 3 degrees every hour. That the temperature of the body be constant is an indispensable condition to the health of the warm blooded animals.

What are the means by which nature arranges to supply any insufficiency of the interior heat, to eliminate any harmful excess, and to bring back the temperature of the organs to a degree that suits the accomplishment of the regular phenomena of life? These means are various. When the nutrition becomes insufficient, calorification is produced at the expense of the tissue of the animal, that we then see becomes thin; the herbivorous becomes temporarily carnivorous. When heat is produced in excess, the organism gets rid of it by a number of issues; in fact, the body is cooled by radiation, by evaporation and by conduction or contact. It is admitted that in ordinary weather one-half is carried off by radiation, the two other means taking each a quarter of the surplus heat.

Evaporation is the safety-valve that regulates the loss of heat in completing to a certain point the action of conduction and radiation. Thus, a large part of the caloric produced in excess is carried off by radiation. The intensity of this radiation by which the heat of our bodies is dispersed around, is proportionate to the difference that exists between the proper temperature of the body and that of the surrounding medium; it is augmented near an object that is very cold. For example, we explain thus the sensation of cold that persists, after a fire has been lighted in a room that has not been heated for a long time, notwithstanding that the temperature in the room may have reached 20°C., whilst it will be comfortable with the thermometer at 17° after a continued warming. It is because in the first case, that the walls and the furniture are still cold and abstract much heat in provoking radiation from our body. The loss becomes less, and the sensation of cold passes off, when the surrounding objects have reached a temperature in the neighborhood of 15°C. We can thus understand how dangerous it would be in winter to remain long seated near a wall or a window, that would cool only one side of the body by excessive radiation.

It is for a reason analogous to the above, that we feel too warm in a hall filled with people, when the temperature does not mount higher than 20°C. Because, the presence of a large number of people, all

having a temperature of 37°, impedes the lateral radiation, the excess of heat is only carried off by the currents of air and by a more copious perspiration. We then fan ourselves to increase the coolness by convection and by evaporation; by bringing a greater quantity of air in contact with the skin. If we leave the hall and go to "take a breath" in another room that has remained empty, we are astonished to see that the thermometer is nearly the same as in the hall; we feel cooler because the body radiates more freely.

The human body is cooled by convection in heating the air that surrounds it, and the loss will be perceptible in proportion to the temperature of the air and to the rapidity of its renewal.

The atmosphere, no matter how calm it may appear, is always agitated by a thousand different movements that are not perceptible to our senses, because, unless a current of air moves at least one metre a second, it makes no impression on our senses; a movement of 0·5 m. will not be felt; we can prove it by agitating gently our hand. An indirect proof of the existence of these currents, is the rapidity with which perfumes spread around in a calm atmosphere.

In our climate the medium rapidity of the currents of air that plough the free atmosphere is estimated at about 3 m. a second, or about eleven kilometers an hour. In admitting that the surface of the body exposed to the current of air is equal to 1 m. square, there passes over a man promenading outside, 11,000 cubic m. of fresh air in an hour.

It is astonishing how little civilization has done to aid us to combat heat when the most various and efficacious means have been invented to preserve us against cold. The Hindoo reduces the production of interior heat by taking little nourishment; but he has no energy and his capacity for work is very small. Assiduous work demands a larger amount of food, and from it results an amount of heat that is harmful, for the organism can only convert into mechanical work 25 per cent. of the increase of heat, that it produces during a sustained effort. It is not, therefore, the production of less heat we should try to effect, but how to relieve ourselves of that which is produced.

As a refrigerant, water is much more effective than air, because of its greater conductibility—at an equal temperature a bath of water will cool us much more than one of air, but the usage of baths is necessarily limited. The question of real importance is how to cool the air that comes in contact with our bodies.

We have yet to consider the pulmonary and cutaneous evaporation. In the tropics when the thermometer is higher than 37°F. in the shade, the body can no longer cool itself by contact or by radiation; there then only remains one way for the surplus heat to be carried off; it must be expended in vaporizing the water that is brought to the pulmonary mucus membrane and to the skin by transpiration.

As a general rule, half as much water is exhaled from the lungs as is excreted by the skin; in repose, the body loses by these two ways 900 grams of water that transforms itself into vapor, but this quantity can be doubled or tripled under the influence of on excess of interior heat, as the transpiration will then open its flood gates. Thus the vaporization of a kilogram of water at 37°C. absorbs 580 heat units; by transpiration at least 500 units are carried off in the 24 hours, that is to say, an amount of heat sufficient to make 5 litres of water boil. The vapor that is thus disengaged diffuses itself in the surrounding air, and the further the air is from its point of saturation the greater will be the facility with which the vapor is absorbed.

Indeed, for a given temperature there exists always a limit to the proportion of vapor that the air can contain; when it has reached this point, it is called saturated. At 37° a cubic metre of air contains 44 grams of water in a state of vapor, at 30° it can contain 30 grams, and at zero 5 grams only. From this we see that a cubic metre of dry air heated in our lungs to 37°C., can carry off 44 grams of vapor of water. But suppose that the air we are breathing is at zero and saturated with humidity, (5 grams to the cubic meter). When heated by respiration to 37°C., it can yet contain 39 grams; the difference will not be perceptible, but if the air is at a temperature of 30°C. and saturated (containing 30 grams of vapor of water to the cubic metre of air), it can only absorb 14 grams in place of 44 when the temperature will be raised to 37°F., we shall only lose 8 units in place of 25, and for 10 centimetres of air that we breathe in the 24 hours there will only be 80 instead of 250 units, making a difference of 170 units.

A hot and damp atmosphere feels so close, because it impedes the evaporation of the water brought by transportation to the surface of the body. Even the wind loses its power of drying. This is why a damp and hot climate is so much more unhealthy than a dry and hot one.

The lightest veil is something of a vestment, as it serves to moderate the loss by radiation from a naked body. It is in the same way that

a cloudy sky in the spring protects the earth from being too much cooled, the dew will only fall when the clouds are absent. In putting on numerous coverings, whose thickness we increase according to the rigor of the seasons, we succeed in diminishing the radiation from our bodies, as if by a series of stopping off places or relays.

Our linen, underclothes, and our cloaks, make up numerous artificial epiderms. The heat that is given off by the skin goes to warm the superimposed covering; the worse the conductor, the slower the heat will pass through; when it arrives at the surface it goes off, but without our perceiving it, as in direct contact of the air we should, since it is not we but the clothes that get cold. What renders our clothes a protection to us is their being wadded with a layer of warm air, the temperature of which is maintained at between 20° and 30°C. Each of us thus has his own little atmosphere that follows him everywhere and renews itself without getting cold. An animal, in its fur, has its couch of air in the interstices, that increases the protecting power. Furs, soft stuffs and feathers owe their warmth to the air that they contain.

It is evidently a question of great interest to determine by experiments the facility with which the various stuffs used as clothing are traversed by heat. The celebrated Count de Rumford was the first who particularly studied these experiments. He used a glass ball with a diameter of 0·04 m., surmounted by a tube through which he introduced into the ball a thermometer surrounded by the substance to be examined. The ball was first put in boiling water, and then into a cooling mixture, and he noted the time that it took the thermometer to descend from 70°R. to 10°R., or to loose 60° Réaumur (75°C.) When the thermometer was bare it cooled in 9·5 minutes. Where it was covered with linen it took 13 minutes. Covered with other kinds of stuffs it took periods of time longer and longer to cool; flax or cotton thread, 14 or 15 minutes; silk or woolen thread, 15 or 16 minutes; flax lint, cotton wadding, 17 minutes; sheep's wool, 18½ minutes; raw silk, 21 minutes; eider down, hare's fur, 22 minutes. These experiments were made in 1798.

The most recent experiments were made by Dr. Krieger. He used a sheet-iron cylinder filled with hot water, which he covered with different stuffs, noting their effect in impeding the cooling of the water. In investing the cylinder successively with a covering of wool, buckskin, silk, cotton, flax (always taking note of the temperature), he only found an insignificant difference, not exceeding 1 or 2 in 100. The

color made no difference. From this it seems that, as long as we use obscure heat, the emissive power, and the absorbing power, which is correlative of it, varies very little in the different stuffs. It is very different if we use luminous heat, or solar rays. With coverings of flax, cotton, flannel, silk, M. Krieger noted the absorption of heat in the proportions as shown by the following numbers: 90, 100, 102, 108. The influence of color was much greater; for cotton stuffs, differently tinted, he found the following numbers:

White	100	Dark green	168
Straw-color	102	Turkish red	165
Yellow	140	Light blue	198
Light green	155	Black	208

To have an idea of the part played by the conductibility of tissue, M. Krieger ascertained in what measure the loss of caloric was diminished by doubling the various stuffs placed around the iron cylinder. He found that satin, cotton stuffs and fine linen, only diminished the loss from 3 to six per cent.; for buckskin, flannel and rather thick cloth, the loss was lessened from 10 to 20 or even 30 per cent.

The clearest result of these experiments is that the resistance offered by these various stuffs does not depend so much on the conductibility of the textile fibres that form their substance, as on their thickness, volume and texture.

A coat that is wadded is much warmer when new, than it is after the wadding has been flattened by its use, as it is thus rendered a better conductor. If the doubling of the covering of the cylinder has little influence when they are wrapped tightly around, it is very different when a space of ½ or 1 centimetre has been left between the two layers. We find, in deducing for the conductibility of the two layers, a retardation of the cooling amounting to 30 or 35 per cent., that is caused by the interposed layer of air, as it is independent of the nature of the covering. From this it follows, that in certain cases, a garment will keep us warmer if it is large, than if it was tight; we know that tight gloves or shoes protect us badly against the cold. But in this reasoning we suppose that the layer of air is immovable; a large, flowing garment is really cooler, being favorable to the circulation of the air. The most serious obstacle that the propagation of heat can encounter in a body, is the discontinuity of its elements. In the manufacture of the various tissues destined to clothe us, these principles are profited by, more or less unknowingly. Very warm clothes are obtained from

stuffs that are light, spongy and loose, because they can contain a large volume of air in the interstices between the fibres; I said contain, but it would be more correct to have said, allow to pass. Indeed, the warm air that surrounds our bodies is not immovable, it is renewed by filtering through the coverings that we think are destined to isolate us from the surrounding medium. A condition that is essential for a good garment is, that it allows of ventilation. The warmest stuffs allow the air to pass through them more freely than those that are considered the coolest. M. Pettenkoffer has demonstrated the proof of this, in measuring the volumes of air that passed through a series of tubes, closed with different kinds of stuffs, the same pressure and time being allowed for each kind of stuff; the following numbers will give an idea of their relative permeability:

Flannel	100	Heavy cloth	58
Linen	58	Buckskin	51
Silk	40	Glazed skin	1

Flannel is one hundred times more permeable than a glazed glove, yet we know that it is infinitely warmer. In using double layers the volumes of air were only slightly modified. Thus we see that our clothes are continually aired, and the activity of the exchange depends on the exterior temperature, the agitation of the atmosphere, and on the porosity of the tissues; what is essential is, that the exchange should be made so slowly as to be imperceptible.

The warmest garment is a fur pelisse; but it is not only the skin, it is above all the hair that keeps the heat, even if its mass should be comparatively insignificant; the heating efficacy of this kind of apparel is due, above all, to the interposed air. M. Krieger has an interesting experiment on this subject; he noted the loss of heat by his calorimetre first, when surrounded with fur in its natural state, then with the skin shaved, and lastly, the same skin was coated with a varnish of linseed oil, then with one of gum-arabic; the loss in these four cases is shown in the following figures: 100, 190, 258, 296. The cleansed skin lost twice as much heat as when it had the hair on, and the loss was tripled by the varnish that closed the pores.

Dogs and rabbits have been killed by being shaved and then varnished; death being caused, not so much by the suppressed respiration as by the loss of heat.

M. Krieger found that when he shaved a rabbit and wrapped it in wet linen and left it in a room at a temperature of 19°C.; the tempera-

ture of the animal fell in five hours from 39·8°C. to 24·5°; in the same time the frequency of the respiration fell from 100 to 50 inspirations a minute. On putting the rabbit in a cage heated to 30°, it was quickly revived.

Air-tight garments are in general unhealthy, being an obstacle to the aeration of the clothes underneath. They are useful to protect us from the rain, but they excite perspiration and prevent it from vaporizing.

An important quality of stuffs is their hygroscopicity. All tissues are hygroscopic; they condense the atmospheric humidity, and the more the atmosphere is saturated the quicker they absorb the moisture, the air being the less capable of vaporizing it. This condensation takes place particularly when the temperature is lowered. When water is absorbed by any cloth, it is divided into two parts, one that can neither be felt or expressed, which is the true hygroscopic water; the other, that fills the pores, can be gotten rid of by compression, it has been called the interposed water. Linen was found to be more hygroscopic than canvas, and hemp more than cotton.

M. Pettenkofer experimented with a piece of linen and a piece of flannel of equal size and weight. They were first dried at a temperature of 100°C., then exposed together in places more or less damp; after being exposed several hours they were weighed, the variations of weight being noted. He found that the wool was twice as hygroscopic as the hemp; but the hemp absorbed much quicker than the wool, it dried also much quicker.

The quantity of water that can be absorbed by the different stuffs is much greater than is commonly supposed. A woolen garment weighing 5 or 6 kilograms can absorb nearly a litre of water, which will add one kilogram to its weight, and to completely vaporize this water, the body would lose 500 or 600 calories! We know that tissues absorb more when the temperature is low, we also know that when the tissue is wet it conducts much better than when dry, and in consequence protects us much less against the cold; this is the great danger of a damp cold.

It may be asked how wool will protect us more from the dampness than linen, being as it is more hygroscopic? In the first place, because it absorbs and gives off water much slower, and also on account of its indestructible porosity. In proportion as the water fills the meshes and pores of a tissue, the less permeable it will be to the air;

the stuffs that have close meshes, such as linens, cottons, and silks, are affected in this way much quicker than woolen stuffs. The elasticity of the fibres has a great deal to do with the persistence of this porosity; the fibres of wool, even when wet, lose very slightly their elasticity and thus keep the pores from closing, whilst the filaments of flax, cotton or of silk, become softened under the influence of the dampness and offer no resistance to the entrance of water. It is for this reason that wet flannel is so much warmer than wet linen. It is true that silk or linen shirts are cooler than woolen, because they soak up more completely the sweat and let it evaporate.

Hygienists in speaking of the different stuffs, vaguely classify them in the order of their "conductibility," designating by this word the greater or less facility with which they appear to be traversed by heat. They admit that the conductibility decreases in the following order: hemp or flax, cotton, silk, wool. Tissues made of flax, hemp or cotton have the reputation of being the coolest; they are easily wet, and cool the skin by conductibility and by evaporation.

"Linen made of flax or hemp," says M. Bouchardat, "are, of all the stuffs made for clothing, the most favorable for the production of those affections that result from the effect of dampness against the skin."

Cotton stuffs let less heat escape, absorb and retain a part of the transpiration, and cool less quickly by evaporation; their use is more advantageous in general than that of linens. A very widely-spread opinion is that cotton wear is not so healthy as linen or hempen stuff; this opinion or prejudice arises from the fact that it is not so good a conductor, and being prickly with asperity, it irritates more the skin. Examined under the microscope cotton fibres appear angular and stiff, whilst the fibres of flax are round and sleek. Cotton is not suitable in cutaneous affections; in this case wool being more downy and warmer than cotton would be still more hurtful. "It must be from this," says M. Bouchardat, "that the above prejudice originated, and it is the only case where any other substance than flax or hemp, that is well washed, very fine and well rubbed down, could do no harm."

With the above exception, cotton has the advantage of linen, in being warmer for winter, and also during summer, in not allowing the body to cool too quickly. Inhabitants of cold, damp countries should use cotton in preference to flax or hemp stuffs. Wool is more irritating to the skin than cotton, its fibres being so stiff and prickly; the excita-

tion that it produces, when it can be borne, is a therapeutic means that may be used in case where the skin needs a stimulant. Unfortunately the wearing of wool against the skin may become the source of the infirmities for whose cure it is indicated, when from too tender an education, the habit is contracted too soon and without reason. From it will result easily a troublesome predisposition to colds, rheumatism, and neuralgia; the habit when once acquired cannot be given up without danger. But the use of wool is precious in certain countries and in certain conditions of life.

Prof. Brocchi attributes the health and vigor of the ancient Romans to their custom of wearing coarse woolen garments. Woolen garments are considered as excellent preservatives against malaria. In the English army and navy soldiers garrisoned in unhealthy places are obliged to constantly wear wool against their skin, and also to wear sufficient clothing, to protect them from paludean fevers, dysentery, cholera, and other diseases. These measures were found efficacious in protecting the health of workmen raising dykes, opening canals and ditches in marshy ground, whereas before the enforcement of these measures, the mortality from fevers was considerable.

M. Balestra thinks, that in exciting the cutaneous secretions, flannel helps to eliminate from the body the paludean miasms that have been absorbed by the pores, and also to get rid of the deposits that cause rheumatic affections. This hypothesis is confirmed by the singular connection that seems to exist in those climates between rheumatic and intermittent fevers. In addition to which, woolen stuffs, on account of their naps, stop some of the germs carried about by the air, which arrives at the surface of the body filtered and purified. M. Balestra has ascertained this filtering power of thick and shaggy woolen stuffs by direct experiment in paludean regions. It seems useless to add that these protecting garments should be often washed. After woolen stuffs, come cotton, still preferable to linen, because they excite the skin slightly. Silk is also warm to the skin. It will do as a substitute for flannel in winter; it will be borne with difficulty on the skin in summer, because of the excessive heat that it provokes. People inhabiting unhealthy countries should never go out without some woolen covering, in provision of atmospherical changes. It is not less important to be well covered during the night; it is a precaution recommended to all those who live in marshy places.

We are sometimes astonished to see the inhabitants of some hot

countries overcharged with woolen garments: the Arab always covered with his *bournous*, or the Spanish peasant with his cloak, the color of tobacco. These garments protect them from the rays of the sun and from the freshness of the night; they are excellent heat regulators.

The head-dress completes the clothing, as a roof crowns a house. It preserves us from insolation, from cold, and from accidents. A hat should be light and well aired. From the experiments of M. Troupeau, the coolest hats are those made round and conical, they are preferable in hot countries to low-crowned ones. The high silk hat, although not very picturesque, is nevertheless a head-dress eminently hygienic and appropriate to the climates of Europe; it covers the head with a couch of air that protects it efficaciously. As to the feminine head-dress, now-a-days, it is the hair that constitutes the essential part.

The house also, is like a vast and ample garment, destined to regulate our connection with the surrounding medium, and to free us from its tyranny, but not to isolate us from it. It should not, or more likely—for it is too often forgotten—it must not deprive us of air. Happily there is no voluntary prison so well corked that the outside air cannot find access to us without our knowing it.

The fact that water penetrates easily through a wall or ceiling is well known to all the world; the spots that form warn us sufficiently. But the air that comes through cannot be seen, and we imagine naturally that none does come through. It is an error; walls do not hinder us from remaining in communication with the exterior air, even in making no account of the joints of the windows and doors, through which continual currents of air are passing. Anyhow, why should not a subtle gas find its way where water could? We are sure that this porosity of walls is not a misfortune, far from it, as we shall see, it is necessary to keep dwellings from getting damp.

A very simple experiment will serve as evidence of the permeability of constructive materials. Mr. Pettenkofer takes a cylinder of dry mortar, $0^m, 12$ long and $0^m, 04$ in diameter, coated all over except the two circular bases, with wax, on the two bases are cemented two glass funnels, one of which is prolonged by a rubber tube, the other terminating in a very fine orifice. In blowing in the tube, air is forced through the cylinder, sufficient to blow out a candle placed at the other extremity. In this experiment the air that has passed through the

cylinder is concentrated in the narrow canal of the funnel, and its rapidity is augmented by this.

The experiment can be varied in the following way: On a base, inaccessible to air, is constructed with bricks and mortar a segment of a wall, the anterior and posterior surface of which will be covered with two sheet-iron plates, each having a hole in it, with a tube inserted, the three other sides of the segment receive some impervious covering; if we blow into one of the tubes a current of air will come out of the other. The same result is obtained with wood and the different kinds of stone that will let air pass through them; some other kinds like compact calcarious stone, are only very slightly permeable. It is true that in walls made with calcarious ashlar, water makes up a larger proportion than in brick walls ($\frac{1}{3}$ and $\frac{1}{6}$ respectively) and in this way the equilibrium is established. As a general rule, the more irregular the stones the greater the amount of mortar, and the least regular are the least porous. When wet all these materials become impervious to air. The experiment with the mortar cylinder will no longer succeed after the mortar has been moistened by aspiration in putting the free orifice of the funnel in water. We find also that it is much harder to force water through bricks and mortar, than air; with great difficulty we may be able to make a few drops appear at the free surface. It is then difficult to dislodge water that has entered the pores of a brick; it will only come out by evaporation and very slowly. It will impede the circulation of air in proportion as it fills the pores, and this unfavorable influence of humidity on the permeability of constructive materials becomes more apparent as the grain becomes closer or finer; a remark that has already been made in speaking of the various kinds of stuffs. Thus we see that damp walls allow air to pass through them with difficulty, and M. Märker found that a single day's rain sufficed to diminish, in a striking manner, the coefficient of porosity.

In ordinary weather and when they are very dry, walls transpire; they are incessantly traversed by feeble currents of air, that renew the air of closed rooms, and relieve it of the humidity with which it is charged. The atmosphere of a house is saturated with vapor from the respiration and transpiration of its inhabitants, by the water that is daily used in the household, without counting the dew that is deposited everywhere when warm air from outside penetrates rooms that have remained cold. This humidity that is generated unceasingly, must be

absorbed by the walls so that it may evaporate outside by the action of the sun and wind. It is for this reason that it is a good thing for building materials to be porous and permeable, and not an obstacle to the circulation of the air that should quicken the evaporation. This remark applies especially to northern countries, where the windows cannot be largely opened.

We must be distrustful of the thoughtless innovators who wish to use iron and zinc instead of the stone and wood of our fathers. The imperceptible transpiration of the walls, so important to carry off the dampness, would be suppressed.

The humidity that the walls receive from the external atmosphere in foggy and rainy weather, disappears generally quickly enough under the influence of the wind passing unceasingly over their surface. It is very different with the dampness inside, that is deposited on the walls of badly aired rooms, if the walls are not porous, it is gotten rid of with great difficulty; even heating will not displace it, it will be evaporated by the heat only to be deposited again. This inconvenience is particularly perceptible in recently built houses, whose mortar contains a large proportion of water, and in ground-floors built on damp soil, that becomes impregnated with water by capillarity. This water closes the holes where the air ought to circulate and the wall remains damp, notwithstanding the evaporation that takes place at the surface, and is very harmful to the inhabitants. Damp walls, like wet garments are cold, the water augmenting their conductibility; much heat is also absorbed by the evaporation. From this arises the succession of catarrhs and rheumatism that afflict the unfortunate tenants.

The quantity of water that a recently constructed wall can contain is astonishing. M. Pettenkofer calculated for a house three stories high, five rooms and a kitchen on each floor, say that it took 800,000 kilogrammes of brick to build such a house; the bricks alone would contain 40,000 kilogrammes; the mortar the same amount. We thus find that the masonry of a house of this size would contain 80,000 kilogrammes of water, not an easy thing to be driven out.

Many ingenious contrivances have been invented to dry the walls of newly built houses quickly; the only ones worthy of serious attention are those based on the principles of heat combined with active aeration. The lower the temperature the more air will be necessary. At 10° C. a cubic metre of air, that we must suppose already three-quarters saturated (containing 8 grammes of vapor) can take up only 2 grammes;

to absorb the 80,000 kilogrammes of water contained in the masonry spoken of, it would take 40 million cubic metres of air at 10° C. This volume of air, in a moderate wind could be brought in contact with the exposed surfaces in twenty-four hours; but as it is evident the dampness will only be absorbed in proportion to the rapidity with which it appears at the surface.

Heat combined with a current of air could hasten the drying greatly. In raising the temperature from 10° to 20°C., we increase the evaporation five or ten times; first, because we augment the absorbing capacity of the air (100 centimetres of air, which at 10°C. can only take up 200 or 250 grammes of vapor, can now carry off nearly 1,000 grammes); and, secondly, in raising the temperature we greatly favor the ventilation.

For a dwelling-house, to be safe, its walls should not contain more than 4 or 5 per cent. of free water.

The best way to determine whether the walls are dry enough or not, is first to ascertain the hygrometric degree of the air in the rooms before and after heating.

If the renewal of air is indispensable to insure against dampness, it is still more so to prevent the accumulation of impurities of all sorts that render the air unfit for respiration. All that is necessary to know is, by what signs tainted air is to be recognized, and how much air a man needs to breathe freely in a closed room.

Ordinary atmospheric air contains 21 parts of oxygen and 79 parts of nitrogen, with 0·03 of carbonic acid; carbonic acid then is only found in the proportion of 3 to 10,000. Though the amount of carbonic acid produced by the inhabitants of a large city amounts to many millions of cubic metres daily, the proportion of carbonic acid gets very little above this, thanks to the movements of the atmosphere, and also to the hygienic influence of plants on the atmosphere that take up the carbonic acid freeing the oxygen and absorbing the carbon. Let us see what takes place in a room occupied, such as a school-room. The air changing by the diminution of oxygen, by the pulmonary and cutaneous exhalations, if the ventilation is insufficient, a time will come when the air will be unfit to breathe. It is when the impurities with which the atmosphere is charged become perceptible by their odor, giving rise to the "*malaise*" characteristic of *closeness*. It is generally admitted that when the proportion of carbonic acid has reached 0·001, that this is brought about.

It has indeed been shown that the carbonic acid augments in proportion as the air becomes vitiated, but the malaise that is felt in a close room ought to be attributed to the perishable organic matters that are contained in the pulmonary and cutaneous exhalations. Peclet says that the air coming out of the ventilators of a large hall, full of people, is stinking (*infecte*). The disagreeable odor so characteristic of a close room, is due, according to certain chemists, to a peculiar substance exhaled from the lungs; it has an alkaline reaction and gives off ammonia.

What is really harmful, are these miasmes that smell. The carbonic acid, a relatively inoffensive gas, is only an indication of the progressive change in the air. From the experiments of MM. Regnault and Reizet we know that an animal can live in an atmosphere containing 0·07 of carbonic acid, the proportion of oxygen being maintained at 0·21. We have seen animals perish, when tightly shut up, notwithstanding that the carbonic acid was carried off and replaced by the due proportion of oxygen. Mantigazza has shown that when two birds are placed under two glass bells, and from one he absorbs chemically the carbonic acid, and from the other the organic matters, that the one in the latter resists much longer than the other. M. Pettenkofer was able to breathe several hours in an atmosphere containing 0·01 of carbonic acid without being in the least inconvenienced, but the carbonic acid was not produced by respiration, it was made by a chemical operation.

All this goes to prove that the few thousandths of carbonic acid contained in an atmosphere vitiated by respiration, are not capable of producing the effects caused by such an atmosphere. The oxygen diminishes in about the same proportion as the carbonic acid increases; but the loss of oxygen no longer explains these effects. We may well ask ourselves if a diminution of one per cent. in the proportion of oxygen would be perceptible; would it not be compensated for by a more frequent respiration?

Carbonic acid has often been accused of effects caused by very feeble doses of carbon monoxide or carbonic oxide, coming off from some incomplete combustion, or being reduced from carbonic acid. Carbonic oxide is a true poison, it destroys the vitality of the red blood globules. M. Leblanc found that a dog was asphyxiated in an atmosphere containing ½ per cent. of carbonic oxide and 3 per cent. of carbonic acid, whilst when the carbonic acid was alone used it only produced asphyxia

at 20 per cent. (by volume.) It is to carbonic oxide that the bad effect of cast-iron stoves is to be attributed, as they give it off when their external surface is heated red-hot. Whether the carbonic oxide is due to the permeability of the over-heated cast-iron, to the oxidation of the carbon of the cast-iron, or to the decomposition of the carbonic acid of the air, it has certainly been revealed by analysis, and has given rise to toxic effects, that have been somewhat exaggerated. Carbonic oxide is found in badly prepared illuminating gas, and may become a cause of accidents if allowed to leak.

What is the volume of air a man needs to respire freely? This is a very complex question, about which hygienists have had much controversy. It is clear that the answer will depend on outside conditions, and still more on the limit of variation or tolerance, that is admitted for the composition of normal air. Let us commence with the simplest case, staying in a room hermetically sealed. In this case the volume of air is measured by the capacity of the inclosed space, "the cubic space" conceded to each inmate represents at the same time the air which he can dispose of. The air changing little by little, the proportion of carbonic acid will at last reach 0·001, the allowed amount. The larger the space allowed, the longer it will take to reach this dose. The volume of air in such a case must be in proportion to the length of time the room is occupied. This understood, in taking as a base, the proportion of one hundredth of carbonic acid by volume (1 litre to the cubic metre) and admitting that an adult exhales 20 litres from his lungs every hour, we find that the volume of air to be supplied to each individual is 33 cubic metres. Thus—33 cubic metres of air already containing 13 litres of acid (0·4 by cubic metres) in adding the 20 litres furnished by respiration we have a total of 33; the proportion limit 0·001, would thus be reached at the end of the hour. Therefore the cubic space to be allowed to a person shut up in a hermetically sealed place for one hour would be 33 cubic metres, 66 cubic metres for two, etc. More would be necessary if there were lights in the room, a candle alone using as much oxygen as a man; it is true that a candle does not set free so many hurtful products. If a higher proportion limit will be allowed, the volume of air could be much reduced. It must also be remembered that the change in the air takes place by degrees, and only reaches the limit at the end of the hour.

When a closed space (a hall), occupied by a given number of persons

is subjected to a regulated ventilation, a fixed rule is established; the change in the air having arrived at a certain limit, no longer varies; the noxious gases are eliminated in proportion to their production. The cubic space has no other part to play than that of retarding the time when the fixed rule will be established. If it concerns a place that is to be occupied for a fixed time like a dormitory, this consideration will have some importance, for things can then be so arranged that the proportion limit will not be reached before the end of the time of its being occupied. Another consideration is that a small space crowded would need so rapid a change as to cause a draught of air, which is always dangerous.

In calm weather it is not always sufficient to open the windows of places much crowded, such as the ward of a hospital. It is really necessary to have some artificial ventilation. The best ventilators are large open fire places, which, in winter when everything is shut, are lighted and produce strong currents of air, not perceptable to the inmates, from the various cracks of the doors and windows.

SURVEYS FOR THE FUTURE WATER SUPPLY OF PHILADELPHIA.

By Rudolph Hering, C. E., *Assistant in Charge.*

(Concluded from page 237.)

The East Swamp Creek weir is placed at Sumneytown, a short distance below the turnpike bridge, on the land of Daniel Krauss. Similar in general design to the Perkiomen weir, its overflow is 26·02 feet long, and it is loaded only at the ends of the apron. The height above the bed of the stream is nine inches.

The West Swamp Creek weir is at Zieglersville, about half a mile above Leidy's mill, on the land of J. Daup and George Kunkel. The overflow is 68·60 feet long, and height of crest above bed of stream, 15 inches. It was built a little lower than some of the others, as its position is a short distance below a ford on a township road, and it was deemed necessary to avoid raising the water at the ford. At this weir there is a crib below the apron, in addition to the loading at the ends.

The Northeast Branch weir is about one and a half miles east of Schwenksville, on the land of John Alderfer. The overflow is 64·21 feet long. The height of the crest above the bed of the stream is 19

inches. There is no crib at this weir, the height of the crest above the apron allowing it to be loaded with loose stones all the way across.

On November 20th the crest and bed-log below were taken off this weir, and measurements for the season were stopped. This was done to satisfy the land-owner, named Wagner, who lives about half a mile above, and whose farm ford was flooded by the dam. This lowered the water about 12 inches, and no complaints have been made since. Should Wagner wish to use his ford next season, it will have to be raised by building a causeway.

The Macoby weir is at Green lane, a short distance above the turnpike bridge, and differs from the others, as it is built upon rock foundation. It has two overflows or crests placed to form an obtuse angle, the apex pointing up stream. A post placed at the apex divides the crests. The rock was first worked to a level for the bed-log, which was of 6 inch by 8 inch oak. This was bedded to the rock in Portland cement, and bolted down with inch bolts placed five feet apart. The bolts were sunk one foot into the rock, and were fastened with wedges and brimstone. The outer ends of the weir terminated in posts which were set into masonry walls. The walls are three feet thick, and extend into the bank on one side, and on the ledge on the other side of the stream.

The length of No. 1 crest is 14·99 feet; the length of No. 2 crest is 11·82 feet; and the height of the crest above the bed of the stream is 12 inches.

The same general method of measuring the flow at the weirs was used in all cases, and consisted of observations of the height of the water at a gauge placed on a post at a point about five feet up stream from the weir crest. This gauge was carefully levelled upon, and its height compared with the height of the crest, obtained by levels taken at intervals of three feet over its whole length. At the long weirs two posts were used, one at each end, and, when practicable, two observers were employed, one at each gauge, making simultaneous observations. This was of special benefit on windy days, when the water was ruffled more on one side of the stream than on the other. Comparisons between the gauge-posts and the weirs were made at intervals, to note any possible change.

The weir observations have all been tabled on large sheets of ledger paper, and the resulting flows are now in process of calculation.

(*b*). *Delaware Division.*—While gaugings and weir buildings were

in progress on the Perkiomen Division, visits from time to time were made to the Delaware Division, to get general ideas of the water-sheds, and to select sites for gauging points. On August 2d and 3d the Tohickon was explored from Point Pleasant to a point about four miles above Ottsville, and during the week ending August 11th the Big and Little Neshaminy Creeks were explored for several miles above their junction at Warner's ford; on August 19th another visit was made to the Neshaminy, and on August 27th a trip was made to Point Pleasant, at which place it was decided to build a weir upon the Tohickon.

Work was begun upon the Tohickon weir September 12th. Regular gaugings of the height of the creek began September 13th, and estimates and gaugings of the flow were made at intervals from August 1st, beginning regularly September 25th, at the weir.

Observations of the flow of the Big and Little Neshaminy were made at intervals from September 13th, and regular gaugings of the height of the Big Neshaminy began at that time.

Meter measurements of the flow of the Big and Little Neshaminy Creeks at a point below their junction at Warner's ford were made, beginning September 28th, and continuing regularly till the completion of the Neshaminy weir.

Work began on the Neshaminy weir October 4th, and regular measurements began November 7th.

The weir on the Tohickon, at Point Pleasant, is located a short distance below Stover's Mills, on Mr. Stover's land. It is built on rock in a manner similar to the Macoby weir. The crest is 43·73 feet long, and walls of masonry, about four feet in thickness, built of stone found in the bed of the stream, extend into the banks on either side. Shortly after the weir was completed the volume of the stream increased considerably, and the weir crest was raised four inches higher to properly back the water on the up stream side. This should have been done at first, but the location of the weir so near the race-way of the mill above caused the proprietors to fear that it might interfere with the running of their wheels, and the weir was built as low as possible. They, however, have never complained of the additional height, and probably have never noticed it. The height of the weir, with its four inches additional crest, is 16 inches above the bed of the stream.

The Neshaminy weir is located on the Big Neshaminy, a short distance below the iron bridge at Warner's ford. It is built on rock,

and consists of a bed-log of 10 in. x 12 in. oak timber, bolted to the rock. Between the bed-log and the ledge is a wall of Portland cement masonry, varying in height from 6 inches to 3 feet, the latter being the depth at a pocket in the ledge, at one end. The bolts are sunk into the ledge at least one foot, and pass up through the wall above, into the bed-log. They are one inch in diameter, and about four feet apart. The crest-pieces are fastened to the upper side of the bed-log, and at each end of the overflow is a post, sunk into a wing of masonry which extends into the bank. The length of the crest is 66·79 feet, and the height of the crest above the bed of the stream is 17 inches.

Flash-boards were used at all the weirs in times of very low flow, to shorten the crests and to confine the flow to observable limits. They were made of inch boards, and were fastened to the crest with iron hooks or dogs, so arranged that they could be readily put on or taken off, as occasion required.

The owners of the lands on which the various weirs are placed were in all cases consulted, and their consent to build the weirs obtained before construction began.

The difficulty of finding laborers delayed the construction of most of the weirs. Most of the localities in which the weirs are placed are isolated, and labor was engaged to a great extent from neighboring farms. In the case of the Neshaminy weir more than a week passed, and several gangs of men were engaged, before one was secured that would go to work.

(c). *Lehigh and Upper Delaware Divisions.*—It was considered desirable to gauge the minimum flow of the Lehigh and Delaware Rivers, and this was done in September with the Department meter. The Delaware was gauged at two points: on September 15th and 16th, at a place about one and one-half miles above Portland; and on September 20th, at another point nearer the Water Gap, above the steamboat landing. The Lehigh was gauged at White Haven, just above the upper end of the upper mill pond, on September 19th.

The results of these gaugings show the minimum flow to be as follows:

Delaware River,	September	20th,	697,000,000	gallons	per	24	hours.
Lehigh "	"	19th,	76,000,000	"	"	"	"

At both places well defined points were made, and the heights of the water noted in connection therewith.

(*d*). *Meter Measurements.*—During certain of the high flows that have occurred since August 1st, meter measurements have been made in the Perkiomen and East Swamp Creek. The Perkiomen was metered on August 2d at a point about 500 feet below the foot-log above Green lane, and the station used for metering East Swamp Creek was at the railroad bridge below Perkiomenville, near the junction of East Swamp Creek with the Perkiomen. As a rule the weirs did not fail to act as such during the high flows, and as the rise and fall of the streams occupies but a few hours during summer storms, it would not be practicable to get meter measurements at the point of maximum flow of all of them.

A staging has been attached to Leidy's bridge to use for gauging the West Swamp Creek, but use has not yet been made of it.

In the Delaware Division several storms occurred during October and November which occasioned too great a flow for the Tohickon weir to carry. The rise and fall of the water was carefully noted, and estimates of the flow will have to be made, as no means of taking meter measurements existed. On the Neshaminy the storms caused increases of flow sufficient to temporarily stop work on the weir. Permission was obtained from the Bucks County Commissioners to build stagings under the iron bridge at Warner's ford, but it was decided to wait till next season before building them.

No rating curves of the current meters were in the possession of the Water Department, and they were therefore rated in November at the Fairmount Reservoir, where the water was lowered to a convenient level for the purpose. The rating was done in this manner: The meter was secured to a stiff rod, and the observer, holding it as nearly plumb as possible in the water, walked along the stone curbing, which was but a few inches above the water in the reservoir. The distance through which he walked was divided into lengths of 25 feet, and a second observer noted the time at which the meter passed each point. In this way a record of the steadiness of the motion was made, as well as the velocity. A third assistant, walking behind the assistant with the meter, carried the battery and the register, and noted the number of revolutions. When a sufficient number of experiments at different velocities ranging from 0·3 to 5 feet per second had been obtained, rating curves were plotted, which are now in use.

RAIN GAUGING.

A number of rain gauges of the Signal Service pattern were procured, and placed at various points during the season, as follows:

Zieglersville	July 16
Green lane	July 24
Ottsville	Sept. 1
Pennsburg	" 9
Doylestown	Oct. 5

The gauges at Zieglersville and Green lane were in charge of the party. The gauge at Ottsville was placed in charge of Rev. G. W. Roth, that at Pennsburg in charge of G. H. Hart, and that at Doylestown in charge of Thomas Walton.

Since the party left the field (December 15th), the gauge at Zieglersville has been transferred to Frederick, at a point about three-quarters of a mile from its former location, and is in charge of J. G. Hillsman; the gauge at Green Lane, after freezing and bursting, has been repaired, and is at present in charge of N. S. Renninger.

Automatic Rain Gauges.—Three of these instruments, after the Draper pattern, were ordered to be made during the summer. They are designed to give a continuous graphical record of rain storms, showing the time of beginning and ending, the variations in the rate of the rain fall, and its amount. These gauges were placed at the following points:

No. 1. Thirteenth and Spring Garden Streets.

No. 2. Green Lane.

No. 3. Doylestown.

They were put in operation upon the following dates:

No. 1. September 11th.

No. 2. October 23d.

No. 3. October 18th.

The delay in getting gauge No. 2 into operation was due to difficulty in getting a carpenter to do the necessary work, and to the fact that after it was put in place the clock-work was found to be out of order, and time elapsed before a man could be found to repair it. It has not been in operation since December 15th.

In the case of gauge No. 3, it was originally intended to place it at Ottsville, as being, topographically, a desirable point. This place,

however, was so far away that an opportunity to set up and regulate the gauge did not occur, and it was finally placed in a green-house at Doylestown, where it was expected that the heat from the boilers would melt the snow falling in winter sufficiently fast to insure its efficient working.

The gauges were found to work well as a rule, and tests showed their construction to be good and to warrant accurate results.

Rain gauge observations are in constant progress at the points above mentioned, and in the tabulated reports accompanying this will be found additional records from other points, having some connection with this investigation.

During the first week in December, the crests were removed from the different weirs in both divisions, gaugings ceased for the winter, and on December 15th, the party reported for duty at Philadelphia.

Measures were taken to continue observations of the flows of the streams through the winter months, as follows:

The steam-gauges on the Perkiomen are to be read twice a week, and in case of high flows, often enough to give the maximum heights. These readings are to be taken by J. G. Hillsman.

The gauge above Longaker's dam is read every day by J. Wisler.

The stream gauge on the Tohickon, at Point Pleasant, is observed in the same way as the Perkiomen gauges, by R. C. Stover, and the Big and Little Neshaminy stream gauges are observed by John Kirk.

The dam at Schwenksville, known as Longaker's, has been carefully levelled upon in order that a section of flow in connection with the gauge above the dam can be calculated. The water from the Perkiomen, the Macoby, East Swamp and West Swamp creeks, passes over this dam.

The party is at present at work upon the calculations for the summer flow, but as considerable remains to be done on the calculations of the heavy flows, the averages for the season have not yet been determined.

Very respectfully,

C. S. Gowen,

Assistant in Charge of Hydrographic Work.

C. SANITARY SURVEY OF THE SCHUYLKILL VALLEY.

The work done under this head comprised an inspection of the water-shed for the purpose of ascertaining the various elements contributing to the pollution of the Schuylkill water. Mr. Dana C. Barber, C. E., was placed in charge of the investigation. He entered upon duty December 15th.

As it was winter, the ground covered with snow, and the streams frozen, it was not possible, besides making a general inspection of the water-shed, to do much more than gather information from the local authorities, and from persons in charge of the establishments contributing to the pollution of the streams, which part of the investigation is about completed. An original estimation or gauging of the quantities of objectionable matters was attempted only where a fairly satisfactory result could be obtained.

The territory was, for convenience, divided into seven districts, the lower ends of which were just above the principal towns, viz., Reading, Pottstown, Phœnixville, Norristown, Conshohocken and Manayunk, and Fairmount Dam. On the completion of the investigation, therefore, it will be possible to state the total amount of pollution at the water in-take of each, excepting Pottstown, where the local supply is now derived from a point on the river below the town.

The report of Mr. Barber, a summary of which is appended, gives an account of his researches in full. He begins at the headwaters of the Schuylkill river, and follows it down to Fairmount Park. The Perkiomen water-sheds above Schwenksville and the Wissahickon valley were not included; the former, because the surveying parties had already been over the ground; the latter, because time did not permit.

The report gives a brief account of the respective water-sheds. The amounts which represent the quantity of domestic waste water, and the disposition of the sewage and excrementitious matter, are carefully stated.

The manufactory waste is given wherever it could be ascertained. It is exceedingly difficult, however, and in many cases impossible, to determine the exact amount of waste water reaching the river. Nothing more can be done in such cases than to record the amount of material used in the various industries, and, by analogy with other similar works, judge of the probable amount of waste. Gauging the effluents

from the establishments, together with chemical analysis, would give the best answer to this question, and it should hereafter be done in the most important cases.

In a few instances, purification of the waste water has been attempted. The sewage from the State Asylum at Norristown is precipitated in tanks, and subsequently filtered, before it is discharged into Stony Creek. Seville Schofield, Son & Co., of Manayunk, and J. & J. Dobson, of the Falls, utilize their waste from wool scouring. The Campbell Manufacturing Company, of Manayunk, filter their dye-house waste water. The efficacy of their processes to purify the sewage and waste waters should be examined into next summer.

Inquiries were also made regarding the death and sick rates in the towns along the river, especially with regard to zymotic diseases; but the absence of Health Boards or Officers made a complete result impossible. Dr. R. S. Keelor has furnished a valuable contribution on this question in relation to Phœnixville. Dr. Chase kindly gave the health statistics of the Norristown Asylum. The United States Census Reports also furnish data on this subject.

The information collected, considering the short space of time devoted to it and the season, is very satisfactory. Some time yet will be required to record the conditions during the summer months.

An examination should be made into the nature of the soils underlying the towns along the river, in order to determine their filtering capacity, and into the disposition of the night soil about the large towns, as well as into the approximate amounts of fertilizers and manure used on the river slopes. To ascertain, if practicable, the quantity of mine water and its acidity would be of value in connection with its neutralization by the lime water above Reading. Further investigations likewise are necessary regarding the pollution from the Reading Gas and several other Works, where at present only imperfect information has been obtained.

The comparative fulness of Mr. Barber's report is largely due to the facility afforded and assistance rendered by the proprietors and managers of almost every manufactory in the valley, many of whom went to considerable trouble in order to give the desired information promptly and thoroughly. In many of the smaller woolen mills, however, considerable difficulty was experienced in obtaining the quantities of materials used; but nowhere, with the single exception of the

Albion Dye Works, in Manayunk, was the desired information positively refused.

Special assistance in compiling the appended statement was furnished by

William D. Pollard, Esq., Secretary and Engineer, Pottsville Water Company.

A. Harvey Tyson, Esq., City Engineer, Reading.

D. F. Reinert, Esq., Borough Surveyor, Pottstown.

Edwin F. Bertolett, Esq., Borough Engineer, Phœnixville.

R. S. Keelor, M. D., Phœnixville.

Alex. K. Calhoun, Esq., Borough Surveyor, Norristown.

D. GEOLOGICAL SURVEY.

Mr. R. H. Sanders, geologist, made the investigations of the geological features of the water sheds and conduit lines.

The reports of the Pennsylvania Geological Survey, Professor J. P. Lesley in charge, furnished valuable information. The line of the Delaware conduit from Kintnersville to the Water Gap is fully described, also the Perkiomen conduit, as far as Norristown, and those portions of the Berks and Lehigh counties which are situated in the Perkiomen water-shed. The geology is alsc mapped along the proposed Lehigh conduit as far as the Lehigh Gap.

Mr. Sanders has so far examined only the line of the Delaware conduit. Prior to recording them on the profiles after these shall have been finished, he describes the formations along its course in general, as follows: "From the Frankfort reservoir to one and one-fifth miles south of the crossing of the Bound Brook Railroad the conduit line traverses a micaceous gneiss, rotten near its surface, from one to fifteen feet deep. From the reservoir to near the Pennypack creek it is covered with gravel, and thence up the valley with soil from one to ten feet deep. On the next half mile is found a hard, massive gneiss coming well to the surface. South of the creek hard, hornblendic rock shows for thirty feet. To the north of it the line passes over mica slate covered with about ten feet of soil, for a distance of fifteen hundred feet; the rock itself is rotten in some places to the depth of ten feet. On the remaining distance to the Bound Brook Railroad chlorite slate is found.

"The Paul's Brook valley, in which the Bound Brook Railroad

crosses the conduit line, first shows limestone for three hundred feet, then sandstone and slate. Over the limestone there is a swamp. From this valley, for about a mile, hard gneisses again appear at the surface. The next fifteen hundred feet shows syenite, sometimes covered with broken rock and loose material from ten to twenty feet deep. Following this, for two thousand feet, is gneiss, mostly hard and massive, but with a few layers of rotten rock extending to a depth of twenty feet. Again syenite appears for about thirty-five hundred feet. It is generally massive and hard, but rotten in places to a depth of five feet. It is covered with from five to ten feet of soil. Thence, up to the Forks of the Pennypack, the syenite, which still continues, is covered with gravel. For fifteen hundred feet above the Forks the rock is probably a horizontal, thin-bedded sandstone, covered with about five feet of soil. After this the Danville sandstone and slates appear, but mostly the latter. The sandstone is thin-bedded and friable; the layers are about horizontal. The same formations extend as far as the Neshaminy creek. At the latter the shales begin to crop out, and continue to the Forks. Near this point they reach twenty feet above the creek, and are overlaid by sandstone. Here the rocks have a slight dip to the northwest.

"At the point where the conduit crosses the Neshaminy the bed rock is slate. The latter extends up the Mill Creek valley, dipping fifteen degrees north, thirty degrees east. Then follows sandstone for several hundred feet, and farther on, for about the same distance, slates appear. Sandstone again forms the rock as far as the Buckingham township line, and dips slightly to the north.

"For the next two miles we find shales and slates, with not over fifty feet of sandstone. The following half mile shows massive, hard sandstone, which is covered with loose material for several feet.

"From a point three-quarters of a mile south of Centerville to one a few hundred feet south of Greenville, limestone appears and is covered with from one to twenty feet of clay. It is followed to the Solebury township line by a coarse conglomerate sandstone. Here red shale crops out; it is but a few feet thick, resting on massive sandstone, which extends down to Centerville. From the mill below Centerville to Lumberville the rocks are alternating layers of slate and sandstone, dipping ten degrees to the northwest. From the Plumstead township line, for fifteen hundred feet north, there is massive red sandstone; then, for a thousand feet, altered slate rock, a very hard mate-

rial to quarry. This is followed by fifteen hundred feet of trap, and again by altered slate to a point about half a mile beyond Point Pleasant. The following two thousand feet show sandstone, then fifteen hundred feet of slates.

"Altered slates, with a small amount of trap, once more appear for about a thousand feet. The next mile and a quarter brings slate, with a small amount of sandstone, succeeded by fifteen hundred feet entirely of sandstone. From here, passing Erwinna to the Roaring Rocks, slates and shales are found, with a few feet of sandstone; then a large mass of trap, extending down below the grade line of the conduit, is found to extend as far as half a mile south of Kintnersville. By keeping the conduit nearer to River Bluff, between Uhlertown and Kintnersville, the trap rock could be avoided, and the tunnel would pass through alternating beds of slate and sandstone, but be about half a mile longer. From the end of the trap down to the run which passes Kintnersville, the rocks are slates and shales.

From this point to the Water Gap the State Geological Survey covered the ground. From the map in Report D, 3, we find the following general profile:

One mile of gneiss, one and a half miles of limestone, one mile of gneiss, two and a half miles of limestone, two and a half miles of gneiss, then again limestone as far as Easton. From Easton, for two miles up the river, occurs limestone, and then, for a mile and a half, gneiss, serpentine, etc. Limestone then appears as far as Belvidere bridge, and up to one quarter of a mile south of Portland we find slates. Extending to half a mile north of Portland is limestone covered with gravel. From here to the Water Gap are slates, and the Gap itself shows hard massive sandstone.

In making the reconnaissance of the Lehigh water-shed above White Haven, Mr. Berlin found that the rock consisted almost entirely of Pocono sandstone and conglomerate, with an occasional outcrop of red shale, making, therefore, an excellent formation for the gathering of water.

The rocks in the Perkiomen water-shed are estimated by Mr. Sanders to be five square miles of limestone, twenty-five miles of quartzites and the remainder red sandstone and red shales with some trap. The water divide from Green Lane to within two miles of the South Mountain is made by a trap dike which has altered the red Shales on

each side of it for about one thousand feet. The rocks in the upper Perkiomen are mainly horizontal.

About sixty square miles of the Perkiomen water-shed have been carefully investigated by the Pennsylvania State Geological Survey, Vols. D, 3. Large areas of the Delaware and Lehigh water-sheds above the respective "Water Gaps" have also been reported upon in detail.

Prof. James Hall, Geologist of the State of New York, has communicated to the Department geological data regarding the Delaware water-shed in that state. The affluents have their origin chiefly in the Catskill or Old Red Sandstone formation. It is quite uniform in character, consisting of coarse and fine sandstone with shaly partings or with intervening beds of shale. A large portion of the area is still a wilderness, or but very sparsely settled.

Special thanks are due to Prof. J. P. Lesley, for advance sheets of maps and reports issued by the Geological Survey.

E. COLLECTION OF SAMPLES.

Samples of water for analysis were from time to time collected and forwarded to Prof. A. R. Leeds. The manner of the collection was in accordance with the instructions received from him.

The samples collected on the Perkiomen were from Perkiomen Creek at Green Lane, at Frederick and near its mouth ; West Swamp Creek at Zieglersville ; Macoby at Green Lane ; East Swamp Creek at Perkiomenville ; North East Branch near Schwenksville ; Skippack at Evansburg, and Stony Creek at Norristown. All water from these localities would have to be impounded.

On the Delaware water-shed the samples were taken from Pennypack at Shelmire's Mills, Big and Little Neshaminy at their junction, Tohickon and Haycock Creeks above their junction, and Tohickon Creek at Point Pleasant,—all of which water requires impounding. Samples were also taken from the Delaware river at Point Pleasant and at the Water Gap.

Samples of water also collected from the Lehigh river at White Haven, and the Tobyhanna above its mouth, representing the quality to be obtained by the Lehigh project.

Finally, samples were collected from the Schuylkill river above Phœnixville, and from the Roxborough and Fairmount pools.

Hand specimens of some of the rocks situated along the lines of the proposed conduits, especially near the tunnels, have been filed at the office.

Boiler scale has been obtained from various localities over which the surveys have extended.

F. OFFICE WORK.

As the fair weather continued almost to the end of the year, and the surveys were carried on as long as possible, very little office work has yet been done.

Quarters were obtained, by authority of the Department, December 20th, at 925 Walnut street, and after a short leave of absence during the holidays, the party began working up the field notes.

The topography along the conduit lines is being plotted to a scale of 200 feet to the inch, and the general water-sheds to a scale of 400 feet to the inch. The latter is finally to be reduced to the scale 1,600 feet to the inch, of the topographical maps of the State Geological Survey, which cover 63 square miles of the Perkiomen water-shed.

The topography will be indicated by 5 feet contours on the conduit plan, and 10 feet contours on the general plans.

A preliminary map has been compiled, on a scale of $4\frac{3}{4}$ miles to the inch, from the best accessible sources, covering the entire Delaware, Lehigh, and Schuylkill water-sheds, to be used for general purposes. Four copies are attached to this report: one showing the areas covered by the surveys of last year; a second, the stations where the streams have been gauged and the rainfall observed; a third and fourth give the rainfall for the third and fourth quarters of the past year.

A general map, comprising the Perkiomen, Tohickon, and Neshaminy water-sheds, and the territory as far south as the city, on a scale of one and a half inches to the mile, will soon be commenced, and the surveys as finished will be plotted on it.

The hydrographic work is being tabulated and compiled.

I beg to make special mention of the industry with which the members of the survey parties have pursued their work during the last season, and especially of the skill with which Messrs. Gowen, Linton, Paddock and Barber have conducted their respective branches of the investigation. The amount of work accomplished during the time spent, as the preceding account shows, is very large and satisfactory.

G. WORK REMAINING TO BE DONE.

The inquiry in regard to a future water supply is of great magnitude, owing mainly to two causes. One is the favorable development of the Point Pleasant scheme, as compared with the Perkiomen; the other is a favorable conduit line from the Perkiomen basin to White Haven, as compared with the Delaware conduit from Point Pleasant to the Water Gap.

Neither of these projects has been seriously considered heretofore. It will not require much additional work to ascertain the main features of the Lehigh conduit line, because the topography of most of the territory is already mapped. But to properly judge of the practicability and advisability of using the Tohickon and Neshaminy water, and of storing it, surveys must be made of these two water-sheds with the same care as those of the Perkiomen valley.

The surveys then in the City's possession would cover the entire ground from which it is possible to obtain water by a near gravity supply. The additional territory to be considered naturally increases the cost of the investigation, yet the sum required is not out of proportion to the expense found necessary in other cities. Boston was obliged to spend about $60,000 for the preliminary investigation for its additional water supply, and New York has expended about $250,000.

The Commission of Experts (1883) say: "Indeed, in other cities large sums are annually expended for surveys to secure fresh information respecting the possible sources of water supply."

To carry the contemplated work to completion would certainly be economical, as the surveying parties have become thoroughly acquainted with the territory and the work, and the final result, as well as the benefit from the sum already expended, could be reached earlier than if the completion were postponed.

To accomplish this the following information will still have to be obtained, after the surveys of last season have been worked up:

Perkiomen Project.—Three square miles of topography for storage basins and conduits.

One hundred and thirty square miles of general topography, to ascertain the physical, commercial and sanitary features, including East Swamp Creek, North East Branch and Shippack valleys.

Sixty-three and three-tenths square miles of water-shed, already contoured and mapped by the State Geological Survey, to be inspected to ascertain its commercial and sanitary features.

Geological examination of the water-shed and the proposed conduit line.

Estimation of land and other damages for conduit and storage basins.

Completion of the statistics of polluting elements in the respective valleys affecting the condition of the surface water to be impounded.

Continuation of the hydrographic work in the basin throughout the year.

Respecting the Lehigh extension, certain parts of Kidder, Penn Forest, Sterling and Dreher townships have not yet been inspected, and a careful reconnoissance at least, is yet to be made of the left bank of the Lehigh, from Mauch Chunk to Catasauqua, for a conduit line.

The collected maps and surveys of the Lehigh water-shed and conduit line remain to be compiled and a general estimate made of the latter from the Perkiomen to White Haven.

Delaware Project.—Five square miles of topography for storage reservoirs in the Neshaminy and Tohickon water-sheds.

Three square miles of topography for alternate conduit lines.

Two hundred and thirty square miles of general topography of the Neshaminy and Tohickon water-sheds, to ascertain their physical, commercial and sanitary features.

Geological examination of the Neshaminy and Tohickon basins.

Estimation of land and other damages for conduits and storage basins.

Compilation of the statistics relating to the condition of the surface water to be impounded.

Continuation of the hydrographic work throughout the year, and of the analyses at important points and times.

Schuylkill Valley.—Completion of the Sanitary Survey of the valley.

Collection of data as to means and cost of guarding against future pollution, by local restrictions and purification works, or by intercepting sewers.

Measurement of the flow of the river by more careful means than heretofore.

Artesian Wells.—Geological examination and borings to ascertain the probable extent of water-bearing strata about the city.

Very respectfully,

RUDOLPH HERING,

Assistant Engineer in Charge.

"THE EARTH'S ELLIPTICITY."

By Pliny Earle Chase, LL.D.

In the July number of the Journal of the Franklin Institute, Prof. d'Auria has amended his first paper on the Ellipticity of planets, by introducing some of the omitted elements to which I called his attention,* and thus obtaining Newton's value for e. Through the hypothesis that polar and equatorial gravitation were equal when the earth's ellipticity was determined, a hypothesis which seems to me altogether untenable, he gets a value nearly identical with Listing's.

In his letter to the Editor (p. 152) he shows, by virtually charging me with begging the question, that he totally misunderstands my reasoning. I do not "accept Newcomb's [Listing's] estimate of the ratio of centrifugal force to attraction at the equator;" but I show that the probability of that estimate is confirmed by the closeness of its accordance with a result which is indicated by known equilibrating tendencies. The centrifugal and centripetal relations which he represents by j and k, being constantly operative, the calculation may be simplified by considering their joint efficiency, instead of treating them separately and modifying one of them hypothetically, so as to bring about an accordance between theoretical and observed values.

Let w_0 = angular velocity of free revolution at earth's equatorial surface, or self-sustaining velocity under combined centrifugal and centripetal tendencies, w being angular velocity of terrestrial rotation. Then $\left(\frac{w}{w_0}\right)^2$ = combined tendency of j and k to compress the terrestrial spheroid. If the earth were at rest we should have $\left(\frac{w}{w_0}\right)^2 = e = 0$ and the form would be spherical; if $w = w_0$, we should have $e = 1$ and the form would be that of a flat disk. The actual value is $\left(\frac{w}{w_0}\right)^2 = \frac{1}{288\cdot4.}$ The most recent estimates of $\frac{1}{e}$ are the following:

Fischer	1868	288·5
Listing	1872	288·5
Jordan	1878	286·5
Clarke	1880	293·5

* *Ante*, p. 20.

I can think of no principle which is more likely to produce and maintain such closeness of accordance than that of "the flow of solids." In each oscillation of semi-gravitating rotation the sum of the gravitating accelerations, gt, represents a centrifugal tendency $(16\cdot98\pi)^2$ times as great as that of mere rotation. Prof. d'Auria may, perhaps, find in this fact and in the immense energy of æthereal oscillation a fruitful field for the exercise of his skill in mathematical analysis.

The harmonic connection of Neptune's mass with that of the earth, seems to be dependent upon the fact that æthereal waves, as well as all other waves, tend to maintain the velocity, $\sqrt{gh}$, which is due to the place at which they originate. The value for the reciprocal of earth's mass, 327994, which was given in the May number of the JOURNAL, was based upon the hypothesis that the earth's orbit was circular; the modified value, 329196, made allowance for the secular orbital eccentricity.

LENGTHS OF INDICATOR CARDS.

By ROBERT GRIMSHAW.

For convenience in reference, I have arranged a table showing at a glance the length of diagram that will be given where the point of attachment of the cord to the main motion lever is $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, etc., as far from the pivot as the distance from the pivot to the point at which the vibrating link is attached to the main lever.

Thus: For indicating an engine of 20-inch stroke, if the "pendulum" is 43 inches long between stationary and swinging centres, if you attach the card $\frac{1}{6}$ of the distance of 43 inches from the fixed centre, you get a card 3·33 inches long. If you attach it $\frac{1}{5}$ the distance, or 8·6 inches, you get a card 4 inches long; and if you attach it $\frac{1}{4}$ the distance, or $10\frac{3}{4}$ inches, you get a closed tracing 5 inches long.

One-fifth the distance would be most convenient on a Thompson or a Tabor machine, or any other instrument having a big barrel, if the speed is not too great, and $\frac{1}{6}$ on a "Crosby No. 2."

It is just as well to have the lengths known beyond any possibility of error in the hurry of calculation; as it is mortifying to find a card

so long as to lap on the barrel and break the pencil point; and it is generally desirable to get as long a card as the speed will allow.

Table of Lengths of Indicator Cards.

Stroke of Piston.	$\frac{1}{10}$	$\frac{1}{9}$	$\frac{1}{8}$	$\frac{1}{7}$	$\frac{1}{6}$	$\frac{1}{5}$	$\frac{1}{4}$	$\frac{1}{3}$	$\frac{1}{2}$
Inches.									
4	......	......	......	......	......	......	......	......	2
5	......	......	......	......	......	......	......	1·67	2·5
6	......	......	......	......	......	......	......	2	3
7	......	......	......	......	......	......	1·75	......	3·5
8	......	......	......	......	......	......	2	......	4
9	......	......	......	......	......	......	2·25	......	4·5
10	......	......	......	......	......	2	2·5		
11	......	......	......	......	......	2·2	2·75		
12	......	......	......	......	2	2·4	3		
13	......	......	......	......	2·17	2·6	3·25		
14	......	......	......	2	2·33	2·8	3·5		
15	......	......	......	2·14	2·5	3	3·75		
16	......	......	2	2·28	2·67	3·2	4		
18	......	2	2·25	2·57	3	3·6	4·5		
20	2	2·22	2·5	2·86	3·33	4	5		
24	2·4	2·67	3	3·43	4	4·8			
30	3	3·33	3·75	4·3	5				
36	3·6	4	4·5						
40	4	4·44	5						
42	4·2	4·67							
48	4·8								

Of course the higher the speed the shorter the card must be, both to prevent "flinging" at the ends and to allow a convenient "pull-out" for hooking on.

Volcanic Interruptions of Telephones.—During the eruption at Krakatoa, telephonic communication was almost impossible at Singapore, the words being drowned by a singular noise which sounded, on a subterranean cable of 1½ kilometers, like pistol shots. The distance between Singapore and the Strait of Sunda, is about 800 kilometers, (497·11 miles). The phenomena seemed to be of an electric nature, rather than acoustic.—*Lumière Electrique*, May 3, 1884. C.

SIEMENS' REGENERATIVE GAS BURNERS.

By Chas. E. Ronaldson, M. E.

[Read for the Author, by the Secretary, at the Stated Meeting held Wednesday, June 18, 1884.

While many improvements in electric lighting have been going on within the past few years, many efforts to improve the quality and the appliances for burning gas have been also made, various burners have been devised, but few of them have been found to possess decided merits, or have come into general use.

Quite lately, however, a decided step in advance has been made, in the construction of a burner for illuminating gas, which involves certain novel, and scientifically correct, principles in its design, and which has so fitly demonstrated its superiority in point of economy and quality of light afforded, that, although but a comparatively short time has elapsed since its first introduction in this country, it has come into very general use, and it is probably simply a question of time when it will be as widely and favorably known and used in the United States as in Europe. The burner referred to, is the Siemens' Regenerative Gas Burner, the invention of which is due jointly to the late Sir William Siemens and his brother Frederick Siemens, of Dresden, although it has been perfected by the latter, who describes the principle of his burner as consisting in heating to a high degree, both the air and gas previous to their combustion, and utilizing for the purpose the heat still remaining in the waste products of combustion.

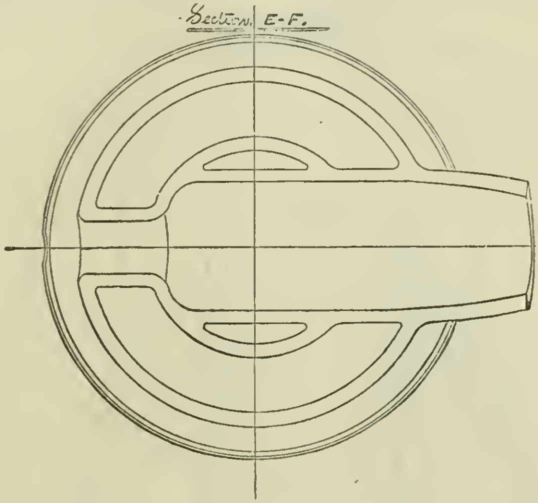
The great merit of these burners resides in the fact that they afford a powerful illumination but at a greatly diminished consumption of gas. They are made to yield 10-candle power per cubic foot of gas burned, while the ordinary burners yield only from 3 to 4-candle power. At the same time, they act as excellent ventilators, when used in-doors.

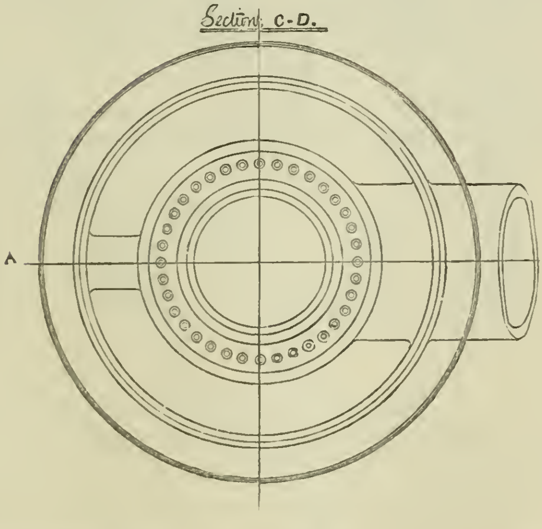
The accompanying illustrations correctly exhibit the construction and operation of these burners, viz.:

A vertical section on the line *A B*; and two horizontal sections upon the line *CD* and and *E F*, show the air chamber, the gas chamber with the tubes at the top of it, and the outside casing.

The action is as follows: The gas passes into its chamber from the pipe at the bottom of the burner, thence up the tubes, meeting at the

point of exit, the proper proportion of air which has likewise passed from below, up through its chamber to the top. Ignition takes place at this point, and the flames (indicated by the arrows) passes upwards

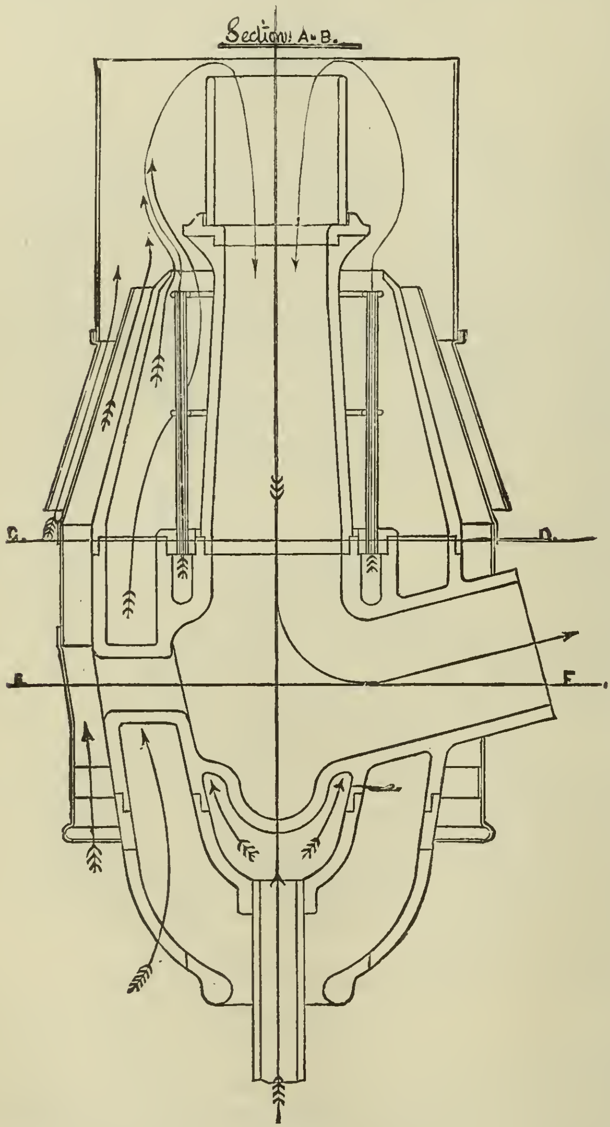




and slightly inwards, around and over the top of the porcelain cylinder, down into the regenerator, thence, through the lateral pipe, to the chimney.

The heat thus generated is absorbed in its passage through the

regenerator, and is in turn taken up by the currents of cool gas or air entering their respective chambers, heating them to above 1,600°F.;



the estimated temperature of the waste products of combustion in the regenerator. The illuminating effect is thus enhanced, and the largest

available quantity of heat and light produced from the combustion of gas in the atmosphere is obtained.

As the chimney and side pipe become heated the draught is keener, and the thorough ventilation of the room effected.

Between the metal casing and the burner is a space where a cool current of air ascends, preventing the over-heating of the burner, and at the same time supplying an additional amount of air to the flame.

Upon the top of this casing rests a glass cylinder, which protects the flame from counter currents of air.

A few minutes will suffice to heat the burner, and produce the best effects.

These burners are particularly applicable wherever a large amount of light is required, whether for in-doors, as for churches, hotels, mills or factories, etc., or out-door purposes, as for streets, stations, etc.

Equally good results are obtained by using either coal or oil gas, and late experiments in Pittsburgh, Pa., demonstrate the fact that these burners consumed the natural gas economically, and with a success unapproached by any other variety of burners.

The saving of gas amounts to from 35 to 50 per cent., while the illuminating effect is about three times the light produced in the best Sugg-Argand of ordinary size, the gas consumption being equal.

In summing up, the claims made are not over-estimated, and the following points of merit are deduced, at one time indicated by theory, but now verified by the most practical and rigid tests, viz.:

Unapproachable power of illumination.

Diversified utility for "intense" illumination.

Economy in consumption of gas.

Complete combustion.

Utilization and disposal of the products, and the waste products of combustion.

Absolute steadiness.

Admirable diffusion and excellent ventilation.

The Siemens' Regenerative Gas Burner Company are at the present time making the following sizes of these burners, but will shortly be able to supply burners consuming 100 cubic feet of gas per hour, having a candle-power of 1,200.

Size.	Gas Consumption.	Candle Power.
I	45-50 cubic feet.	450 to 500
IIa	35 " "	300 to 350
II	25 " "	200 to 250
III	14 " "	100 to 125

APPENDIX.

The following tables are copies from the official records of trial tests made with the Siemens' Regenerative Burner by a number of the New York gas light companies. The figures will tell their own story. —C. E. R.

The New York Gas Light Company, New York, January 18, 1883.

DATE.	Standard of gas and burner. Bray's U. S. No. 7.						Friedr. Siemens' Reg. Burner.					
1883.	Candle power.	Specific gravity.	Gas consumed per hour.	No. of burners.	No. of experiments.	Duration of test. Min.	No. of observations.	Pressure on burner.	Gas consumed per hour.	Observed candle power.	Candle power per foot of gas consumed.	Time of burner lighted. M.
Jan. 18	26·9	·650	5	1	1	10	20	·9	49·00	396·88	8·10	30
18	26·9	·650	5	1	2	10	20	1·1	53·75	443·80	8·24	60
18	27·0	·650	5	2	3	10	20	·7	28·00	220·86	7·36	25
18	27·0	·650	5	2	4	10	20	·7	28·50	225·45	7·91	50
18	27·0	·650	5	2	5	10	20	·8	29·75	239·76	8·06	75

(Signed) ALBERT T. HALLOCK, PH.D.,

Chemist N. Y. G. L. Co.

The Mutual Gas Light Company, New York, Dec. 27, 1882.

DATE.	Standard of gas and burner. U. S. No. 7.	Bray's Batwing.				Friedr. Siemens' Reg. Burner.						
1882.	Candle power.	Specific gravity.	Gas consu'd per hour.	No. of burners.	No. of experiments.	Duration of test. Min.	No. of observations.	Pressure on burner.	Gas consu'd per hour.	Observed candle power.	Candle power per foot of gas consumed.	Time of burner lighted. M.
Dec. 21	28	·700	5	1	Preliminary test.	10	5	1·25	39·00	269·08	6·90	20
21	28	·700	5	1	1	10	5	1·25	40·00	371·84	9·29	35
21	28	·700	5	1	2	10	5	1·30	40·00	412·72	10·32	50
21	28	·700	5	1	3	10	5	1·45	40·80	413·00	9·88	70
22	28	·700	5	2	Preliminary test.	10	5	1·45	22·40	190·68	8·49	20
22	28	·7[illegible]0	5	2	1	10	5	1·50	22·60	198·24	8·77	35
22	28	·700	5	2	2	10	5	1·50	22·60	212·80	9·46	50
22	28	·700	5	2	3	10	5	1·50	22·30	198·84	8·91	70
23	26	·725	5	......	Preliminary test.	10	5	1·25	11·60	97·86	8·45	20
23	26	·725	5	1	1	10	5	1·00	11·60	97·86	8·45	35
23	26	·725	5	3	2	10	5	0·40	10·30	104·00	9·17	50
23	26	·725	5	3	3	10	5	0·40	10·40	97·86	9·41	70
27	26	·725	5	4	1	10	5	1·8	8·00	69·16	8·64	30
27	26	·725	5	4	2	10	5	1·8	8·00	67·31	8·42	35

The Municipal Gas Light Company of New York, Jan. 4, 1883.

1883.												
Jan. 2	29·9	·653	5	1	1	10	10	3	46·00	407·18	8·85	30
3	29·50	·667	5	1	2	10	10	10	46·56	357·25	7·67	20
3	29·50	·667	5	1	3	10	10	10	46·80	386·15	8·25	50
4	29·37	·670	5	3	4	10	10	10	13·14	111·16	8·46	25
4	29·37	·670	5	3	5	10	10	10	13·20	113·66	8·61	50
4	29·37	·670	5	2	6	10	10	10	24·00	209·99	8·76	25
4	29·37	·670	5	2	7	10	10	10	25·20	227·03	9·01	50

(Signed) W. H. BRADLEY, *Chief Eng'r M. G. L. Co.*

The Metropolitan Gas Light Company, New York, January 22, 1883.

DATE.	Standard of gas and burner. Bray's Special No. 7.						Friedr. Siemens' Reg. Burner.					
1883.	Candle power.	Specific gravity.	Gas consumed per hour.	No. of burners.	No. of experiments.	Duration of test. Min.	No. of observations.	Pressure on burner.	Gas consumed per hour.	Observed candle power.	Candle power per foot of gas consumed.	Time of burner lighted. M.
Jan. 22	23·50	0·547	5	1	1	10	10	0·6	48·90	359·32	7·35	25
22	23·50	0·547	5	1	2	10	10	0·6	49·50	400·91	8·09	50
22	23·50	0·547	5	1	3	10	10	0·6	49·80	370·36	7·48	75
22	23·50	0·547	5	3	4	10	10	0·3	14·00	98·70	7·05	20
22	23·50	0·547	5	3	5	10	10	0·3	14·10	104·58	7·42	40
22	23·50	0·547	5	3	6	10	10	0·3	14·04	104·58	7·45	60

(Signed) HERZOG, *Engineer M. G. L. Co.*

The Harlem Gas Light Company. New York, January 13, 1883.

DATE.	Sugg's standard of gas and burner. Argand holes.						Friedr. Siemens' Reg. Burner.					
1883.	Candle power.	Specific gravity.	Gas consumed per hour.	No. of burners.	No. of experiments.	Duration of test. Min.	No. of observations.	Pressure on burner.	Gas consumed per hour.	Observed candle power.	Candle power per foot of gas consumed.	Time of burner lighted. M.
Jan. 13	19·25	0·510	5	2	1	10	10	1	26·10	208·30	7·98	15
13	19·25	0·510	5	2	2	10	10	1	26·10	208·30	7·98	30
13	19·25	0·510	5	2	3	10	10	1	25·38	209·80	8·26	45
13	19·25	0·510	5	2	4	10	10	1	25·38	209·80	8·26	60
13	19·25	0·510	5	2	5	10	10	1	25·90	210·20	8·18	75
13	19·25	0·510	5	2	6	10	10	1	25·80	209·40	8·12	90

The Manhattan Gas Light Company of New York, December 22, 1883.

DATE.	Sugg's "G" burner. 33-hole Argand.					Friedr. Siemens' Reg. Burner.					
1882.	Candle power.	Specific gravity.	Pressure.	Gas consumed per hour.	Candle power per foot of gas.	No. of tests.	Siemens' burners. No.	Gas consumed per hour.	Candle power.	Candle power per foot of gas.	Time lighted. M.
Dec. 22	25	·44	·55	7	3·57	1	1	51·06	327·	6·4	20
22	25	·44	·55	7	3·57	2	1	51·00	505·8	9·91	50
22	25	·44	·55	7	3·57	3	2	26·80	192·	7·16	
22	25	·44	·55	7	3·57	4	2	27·00	208·8	7·72	
22	25	·44	·55	7	3·57	5	3	13·2	72·3	5·48	15
22	25	·44	·55	7	3·57	6	3	14·28	86·34	6·04	50

THE INTERNATIONAL ELECTRICAL EXHIBITION OF THE FRANKLIN INSTITUTE.

The International Electrical Exhibition, for which the Franklin Institute had been making extensive preparations for many months, was opened, according to the official announcement at noon on Tuesday, September 2, 1884.

The formal proceedings were simple and appropriate. They consisted in an address of welcome, by his honor, Wm. B. Smith, the Mayor of the city, to the representatives of the United States, and representatives of learned societies, of foreign governments, distinguished visitors and invited guests, who were assembled in the large lecture hall. Thence the auditors proceeded to the centre of the south gallery in the main Exhibition Building, where, in the presence of a great concourse of visitors, the President of the INSTITUTE, Mr. Wm. P. Tatham, delivered an appropriate address. The formal proceedings were concluded by his Excellency, Hon. Robert M. Pattison, Governor of the State of Pennsylvania, who, at the close of an admirable address, declared the Exhibition open.

The opening ceremonies were attended by official representatives of the British, French, Belgian, German, Mexican and United States Governments, by representatives of numerous scientific and technical societies, the American Institute of Mining Engineers attending in a body.

Although in some important features the Exhibition was not complete on the opening day, the exertions of the managers to have everything ready in time were well repaid by the generally creditable condition of the exhibits.

At the time of this writing the Exhibition is at its height, and it is proper to say that in every important element that goes to make a notable exhi-

bition, the International Electrical Exhibition has fully realized the hopes and anticipations of the managers of the great undertaking.

The presence and attendance of many members of the British and American Association for the advancement of Science, the Royal Society of Canada, the American Institute of Electrical Engineers, and the meeting of the United States Electrical Conference, which was called into existence through the representations of the officers of the Exhibition, has added greatly to the importance and usefulness of the Electrical Exhibition, in that it has enabled the officers charged with the scientific branch of the work to secure the valuable aid and active participation of many distinguished men of science, on the Board of Examiners having charge of the work of tests and measurements of machines and apparatus. This important work is now in an advanced state, and promises, when finished and published, to take rank as an important contribution to applied electricity.

The direct value of the Exhibition as a means of education has been utilized to the highest possible degree. The Board of Public Education of the City of Philadelphia has directed each of the schools of and above the grade of grammar school within its control, to take a day to visit the Exhibition with its teachers, in lieu of a school session ; and, in response to invitations sent by the officers of the Exhibition to the directors of schools within a radius of one hundred miles, embracing Eastern Pennsylvania, New Jersey, Delaware and Eastern Maryland, a large number of pupils with their teachers have availed themselves of the opportunity of visiting the Exhibition. To assist to an intelligible study of the machinery and apparatus, a series of "Electrical Primers" was prepared, giving in simple and readily comprehensible language, explanations of the principles, construction and operation of the principal groups of exhibits, and placards were posted conspicuously in the neighborhood of characteristic machines, etc., to identify them by name to those to whom they were unfamiliar. The primers, which were issued and sold at a nominal price, proved their usefulness by an enormous sale.

The educational character of the Exhibition was furthermore pronounced by a series of lectures given twice a week in the large lecture hall. These lectures, which were delivered by men of eminence upon topics related directly or indirectly to the subject of Electricity, were listened to by crowded audiences and proved to be highly popular. They will be published in a special volume after the close of the Exhibition.

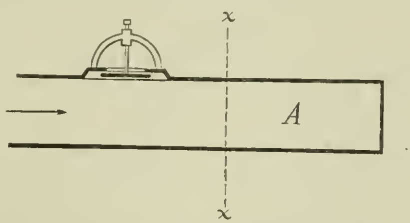
The International Electrical Exhibition, by universal admission, is by far the most valuable and successful (from an educational standpoint) exhibition ever undertaken by private enterprise in the United States, and the officers and members of the Exhibition Committee, through whose earnest, long-continued and well-directed labors the undertaking was carried to a successful issue, are well satisfied with the results of their work. The JOURNAL for November will contain an elaborate historical sketch of the Exhibition by Prof. E. J. Houston, the Electrician of the Exhibition, and a summary of the work of the Board of Examiners, which promises to be of the highest interest, will be given by Prof. M. B. Snyder, Chief of the Board of Examiners. Prof. Snyder will likewise review the work of the United States Electrical Conference. W.

CORRESPONDENCE.

Committee on Publication Franklin Institute.

GENTLEMEN:—Referring to the discussion on "Tests by Hydrostatic Pressure," I beg to say that I do not remember having heard Mr. Nystrom state that my "statement, that the momentum of the water in the connecting pipe can hardly even temporarily increase the pressure in the boiler, is disproved by the operation of the hydraulic ram" (page 121, Aug., 1884). I am so much more surprised at having overheard this remark, as the hydraulic ram was then in my mind, and I would have referred to its relation to my statement if I had not then dismissed the idea that any one of the audience would be thoughtless enough to confound the two cases. In the boiler there is no outlet to the water forced into it, and the additional water increases the pressure *slowly*, because of the elasticity of the walls. If the outlet from the chamber (or termination of the feed-pipe) of a hydraulic ram were *closed*, I fear Mr. Nystrom could not force one drop of water into the same without increasing the pressure very far beyond that due to the impact. Or let Mr. Nystrom make a hydraulic ram using a common ten-horse boiler for the chamber in which the impact is to take effect, I am afraid he would not pump much water.

Let the sketch represent a hydraulic ram without outlet, and I am willing to repeat and defend the statement that the pressure in the space A is a function of the *amount of water* which has been forced beyond the section xx, no matter whether that compression was caused by a steady pressure or by a blow. By concussions of the feed-water no boiler under test ever suffered anything beyond what was due to the pressure owing to the amount of water that had been forced into it, except if a number of regular concussions produced vibrations, which may in rare cases have a slight tendency of producing extra strains.



The calculations of Mr. Nystrom showing the alleged pressure resulting from impact are simply another instance of the anomalies at which theorists arrive who leave out of consideration the principal factors of a phenomenon (in this case the friction of water in the pipe is one of the principal causes of retardation). And why should the retarding force $\frac{2LvA}{Ta}$ be an *additional* pressure? Being a retarding force, expressed in pressure per square inch, it can express the *absolute* difference only between the reaction of the boiler and the pressure of water as it enters the force-pump, and not

a pressure over and beyond the hydrostatic pressure in the boiler. That the impact should produce an additional pressure of 168 pounds, or even 10 pounds, when the boiler is not in a condition to offer an additional resistance, of $\frac{1}{10}$ pound (owing to the quantity of water contained in the same), is simply preposterous. If friction is neglected, the formula (7) is correct enough as far as it goes, but P is the hydrostatic pressure in the boiler, and T is the unknown quantity determinable by the formula.

The phenomenon can be compared with the well-known experiment of the athlete carrying an anvil on which a blacksmith is plying his trade.

HUGO BILGRAM.

Book Notices.

A TREATISE ON TOOTHED GEARING. By Howard Cromwell, Ph.B. New York: John Wiley & Sons.

The author, after having "sought often and earnestly, but always in vain, for a terse, compact, yet complete and comprehensive work on the subject of toothed gearing," attempts to supply this want by the present treatise. A careful perusal, however, shows plainly that he has made a failure of it, apparently because he himself is not fully acquainted with the subject on which he writes. The following are some of the more prominent errors:

His explanation on the cycloidal gearing is rambling, to say the least, when he attempts to base the correctness of form on the least possible friction. The point a of the tooth C' (Fig. 12) describes a prolate epicycloid, not a hypocycloid, in relation to the tooth C, and this can therefore not account for the hypocycloidal form of the flank. The point a of the tooth C describes, in relation to the tooth C' an epicycloid described by rolling the circle O on B, which can never show why the face of the tooth C' should be an epicycloid produced by the circle O'' rolling on B. In Fig 12 the first contact between the teeth C and C' takes place on the pitch-point of the tooth C, which is a blunder. He explains that the point a of the one tooth slides along the flank of the other tooth while the point a of the other tooth slides along the face of the first one. If this were correct there would be at least two points of contact between two working teeth after the first contact.

To the all-important subject of interchangeable gearing he devotes fifty-five words which are casually inserted at a place where nobody would look therefor.

The explanation for the correctness of involute gearing is vague, and one of the essential conditions—the constant proportion, in mating gears, between evolute-circle and pitch-circle—is not even mentioned. He uses the bottom of the teeth for the evolute circle; yet in describing the involute rack he mentions the angle of 75° as the proper one. His statement that radial flanks are used in involute gearing is incorrect.

He figures (Fig. 66) a worm working inside an annular worm gear after stating that worm-teeth are drawn as for a rack. If he had made the worm a modification of a Hindley worm, the absurdity would be less profound.

By placing the square wheel of Fig. 100 inside of its mate, it will become evident that the circumferences of both wheels as designed by the author, are *not* equal, as he says they should be.

If a student attempts to get an insight into the theory of gearing by means of this treatise and fails to see how the assertions correspond with the demonstrations, he can certainly not be blamed. There is, indeed, a serious danger that he may become hopelessly confused on the subject of gearing. H. B.

ELECTRIC BOATS.—M. Reckenzaun believes that on boats of moderate dimensions electricity can be advantageously substituted for steam. The motor should always be ready to act and should occupy the least possible space. The duration of action is of little consequence, since the distances to be traversed are generally small. An electric boat can carry twice as many passengers as a steam boat; consequently it can be smaller, cheaper, and less difficult to drive. Electricity has the additional advantage, especially in pleasure boats, of avoiding smoke, smell, noise, and the inconvenient presence of the boiler. It is not long since a steam canoe carrying fuel sufficient for seven hours' consumption was considered a marvel; electric boats already surpass this limit. During the past autumn an electric canoe made numerous voyages on the Danube, under excellent conditions, and there is reason to expect that even ships of considerable size may be finally driven by electricity.—*Chron. Industr.*, Feb. 3, 1884. C.

PERFECT ELASTICITY OF CHEMICAL SOLIDS.—W. Spring has conducted an extensive series of experiments with the view of determining the possibility of permanently diminishing, by means of pressure, the volume occupied by a given weight of any chemically definite solid body. Prismatic sulphur, when freshly prepared, and plastic sulphur, are changed, under great pressure, into the denser variety of octohedric sulphur; in like manner, amorphous arsenic is partially transformed, under adequate pressure, into denser crystalline arsenic; but in each of these cases there is a change of allotropic state accompanying condensation. Spring therefore concluded that matter takes the allotropic condition corresponding to the volume which it is forced to occupy, and his researches confirm this opinion. Solids behave under pressure like liquids and gases, and the specific weight of a body, when it is pure and free from cracks and crevices, has a signification of the same value as the atomic weights of the elements.—*Bull. de l'Acad. Belg.*, 1883. C.

PROTECTION AGAINST BOILER EXPLOSIONS.—Prof. Melsens in 1871 published some interesting experiments upon heating water in contact with metallic surfaces. Boutigny, in his investigations of the spheroidal state, was led to regard it as the principal cause of the fulminating explosions of

steam boilers. Melsens concluded that when the bottom of the boiler is provided with numerous points, the ebullition is produced with ease and the water does not assume the spheroidal state under circumstances in which that phenomenon would be produced in presence of a smooth metallic surface. M. le Blanc invited Prof. Melsens to exhibit his experiments before the "Sociéte d'encouragement pour l'Industrie Nationale." They were very satisfactory and were highly appreciated by all who witnessed them.—*Bull. de la Soc. d'Encour.*, Nov., 1883. C.

Prout's Law.—Marignac, in his late re-examination of some of the atomic weights, considers that Prout's law is only approximate, and that, since the numbers which express the atomic weights only represent ratios, there is no reason for taking the hydrogen unit in preference to 16 or 100; but the choice of 16 is justified by its practical advantage. It allows us to represent the atomic weights of the greatest number of elements, and especially of those which are most important, by the most simple possible integers, and with the least difference from the rigorous results of experiment. The fact that the atomic weights exhibit more exact ratios to the oxygen than to the hydrogen unit was pointed out by Chase in his 138th photodynamic note (Proc. Amer. Phil. Soc., Nov. 4, 1881.)—*Ann. de Chim. et de Phys.*, March, 1884. C.

Manganese in Wine.—Maumené has analyzed three specimens of wine: 1, a red wine of Grave, of the vintage of 1865; 2, a similar wine of 1882; 3, a white wine of 1883. In the first specimen he found manganese under the form of a tartrate of potassa and manganese. The second wine contained a like compound in a slightly different proportion; the same is true of the third specimen. It is to be observed that the lands where the three vineyards were situated belong to a region in which manganese abounds.—*Chron. Industr.*, April 13, 1884. C.

New Applications of Electrolysis.—Two new applications have been lately reported which seem worthy of further trial. The first is a system of tanning, in which the skins are suspended in a tannin bath, which is traversed by an electric current. The skins are at first placed at the negative pole, in order to destroy the nitrogenous matters; after eight days a more concentrated solution is introduced, and the direction of the current is reversed. This is said to oxidize the tannin and to hasten its precipitation in the cells of the hides. In the second application, M. Abadie seeks to recover the tin from the scraps of tinned iron which are so abundant about tinner's shops. He places them in an electrolytic bath formed of a solution of chloride of sodium with the addition of chlorhydric acid. The anode is metallic and the tin is deposited upon it, in crystals if the current is intense, or in an amorphous layer when the current is moderate. The rapidity and thickness of the deposit is said to be proportioned to the acidity of the bath.—*L'Electricien*, March 15, 1884. C.

Trans-Neptunian Planet.—Flammarion has followed the line of investigation which was marked out by Forbes and has published the indiations which he has discovered of a probable planet, at the first of the

harmonic distances which was pointed out by Chase (Proc. Amer. Phil. Soc., xiii., 140). He admits that many years will doubtless elapse before the planet can be discovered and followed by the telescope, but he thinks that there is no doubt of its existence and that it will appear like a star of the twelfth magnitude, which is quite large enough to be seen by many of the telescopes which are now in use in astronomical observatories.—*L'Astronomie*, March, 1884. C.

First Balloon Passage from France to England.—M. F. Lhoste made his first attempt to cross the English Channel by balloon, on May 27, 1883. After nearly reaching Dover the currents drove him towards the coast of Holland, and he landed at Wönsdrecht. Subsequent unsuccessful attempts were made on June 5, June 7, July 14, and August 13. Not being discouraged by these successive failures, he made an ascension from Boulogne-sur-mer on September 9, at 5 P. M., and he landed on the English coast at about 11 A. M. on the following day. He communicated an interesting account of his several attempts to Flammarion, which is published in *L'Astronomie* for March, 1884. C.

Oblateness of Uranus.—Prof. Saffarik having called Schiaparelli's attention to the favorable position of Uranus for the study of its figure, the Milanese astronomer examined it with powers of 322, 417, 500, and 690. The disc was very sharp and well defined. From 25 series of observations he deduced 3·91″ for the mean equatorial diameter, and 3·756″ for the polar diameter. This gives an ellepticity of 1 ÷ 10·98, which is almost identical with that of Saturn. These measurements confirm those of Madler and Saffarik.—*L'Astronomie*, March, 1884. C.

Hatching Chickens by Electricity.—An interesting experiment has been successfully tried in Berlin. A basket is provided containing a nest of hay, closed by a well-adjusted covering and provided underneath with a thick pillow, which contains a metallic spiral. A battery of six cells, in a neighboring room, furnishes the electricity for warming the spiral, the current passing through a lever regulator. As the temperature should be kept constant, a small thermometer is inserted, with the bulb in the nest and a fine platinum wire soldered into the tube. When the temperature becomes too high, the mercury touches the wire, forming a contact which throws the apparatus out of circuit. When it cools again the current is restored and the temperature raised.—*La Nature*, May 31, 1884. C.

Tide-Gauges.—The sea level has been generally adopted as the base of geodesic measurements. Of late years automatic instruments have been used for registering the mean level of the ocean at stated epochs. These measurements have been made with the greatest exactness in Holland, where the gauges are under government control. The level of the North Sea does not seem to have varied for a hundred and fifty years. The level of the Baltic is the same as when the first observations were made in 1826. The principal European governments have commissions which are charged wirh these measurements. The French Commission has begun a

very important work, which will surpass in extent everything of the kind that has hitherto been undertaken. In Belgium measurements have been for 8,477 different localities. The leveling, which is now in progress in Holland, though on a narrower scale, is remarkable for its precision. In Germany operations began in 1865 and will probably be completed in 1887. In Russia the work has been in progress since 1873. An interesting point, which will soon be determined, is the difference of level between the Baltic and the Black seas. The Spanish operations have shown that the level of the Atlantic at Santander is 0,582 metres above that of the Mediterranean at Alicant.—*Ann. des Ponts et Chauss.*, March, 1884. C.

IMPORTATION OF AMERICAN GRAPES INTO FRANCE.—Ch. Joly has presented a paper to the *Société d'Horticulture*, upon the importation of muscat grapes from the United States into France. The grapes were preserved in an aqueous solution of glucose. The attention of the grape growers of France and Algiers is invited to the important consequences which may follow this importation. The United States, outside of their enormous grain trade, have hitherto been limited to the export of fruits prepared by a previous drying, which removes about 85 per cent. of the water that they contain ; this operation has the double advantage of diminishing the weight to be transported and of facilitating the preservation of the fruit. M. Joly, while calling attention to the danger of an interference with the French grape trade, urges cultivators to increase their production as much as possible, chiefly by the rational amelioration of their methods of culture.—*La Nature*, May 3, 1884. C.

HYDRODYNAMIC IMITATION OF ELECTRIC PHENOMENA.—Decharme, having imitated, by liquid or gaseous currents, the principal phenomena of static and dynamic electricity, electromagnetism, electrodynamics, induction, electrochemistry, and even electrophysiology, concludes that electric and magnetic phenomena are similar to hydrodynamic phenomena ; in other words that electricity, under the form of an æthereal or ponderable current, is analogous to a liquid current, and, when in a state of tension, resembles a quantity of liquid diffusing in a jet. Electric movement is then a true flow and not a mere vibration. Its equipotential lines may be exactly represented by Lamé's differential equation for the curves of hydraulic level. There are some electric phenomena, however, which appear to be the result of vibratory and gyratory movements ; but these movements naturally result from the transformation of wave motion in a continuous current.—*Ann. de Chim. et de Phys.*, April, 1884. C.

TREPANNING OYSTERS.—Bauchon Brandely, in order to study the embryonic growth of the oyster, perforated the upper shells by means of a trepan. After the hole is made and the particles of the shell carefully removed a stopper is prepared of linen, wax or some other material, to exclude the water and the enemies of the oyster. The shell is reconstructed by a thin layer of pearl in about eight days. In order to prevent the growth being too rapid while the observations are going on, the stopper is turned around every two or three days.—*Les Mondes*, April 19, 1884. C.

New Source of Caoutchouc.—The attention of the Indian government has lately been drawn to a new plant, common in southern India, which gives an adundant supply of pure caoutchouc. It belongs to the class of the *Apocynaceae*, and is called *Prameria glandulifera;* it is a native of the forests of Cochin China, and its sap is often used medicinally. When the branches are broken, a large quantity of Caoutchouc is seen in the interior, which can be drawn out in threads, as in the *landelphia* of eastern Africa. The plant can be propagated from young shoots, and M. Pierre, director of the botanical garden of Saigon, thinks that it can be transplanted until it is about ten years old, and that it may constitute an addition of great value to the forest products of India.—*Mon. Scientif.*; *Bull. de la Soc. d'Encour.*, March, 1884. C.

Phosphorescence of the Diamond.—Experiments have lately been made with a diamond of 92 karats, which is one of the finest known outside of royal or national collections. It is of wonderfully pure water and is admirably cut with 64 perfectly geometrical facets. Its value is estimated at $60,000. When exposed during an hour to the rays of the sun it preserved for more than twenty minutes in a dark chamber, a light sufficient to show the white paper which reflected its rays. The same phenomenon was exhibited, but with somewhat less intensity, after having submitted the diamond to a powerful electric light. A very apparent phosphorescence was also produced by rubbing the diamond for a few moments with flannel.—*La. Nature*, May 10, 1884. C.

Pure Magnesium.—Grätzel's patent for the separation of alkaline metals by electrolysis has been very successful in the reduction of magnesium. At a late sitting of the *Electrotechnishe Verein*, in Berlin, a ball of magnesium, of about 15 centimetres diameter, excited general attention. It was of superb brilliancy, similar to that of silver, and had lost nothing of its lustre since its separation by electrolysis. This preservation is a sign of its chemical purity, and forms a remarkable contrast with the magnesium hitherto obtained, which was always more or less affected by potassium, and consequently easily oxidized, especially in a damp atmosphere. Magnesium seems destined to increasing maritime use, for its rays appear to have a greater penetrating power in fogs and mists than those of the electric light.—*Lumiere Electr.*, May 31, 1844. C.

Artificial Graphite.—Dr. Aron has exhibited, at the meeting of the Berlin Electrical Society, various specimens of vegetable carbon, which were made conductors and rendered almost incombustible by energetic and prolonged heating, in a vacuum or in a neutral atmosphere. These properties are so much like those of graphite that the product may well be called artificial graphite, although it is not crystalline. The experiments show that if, as has been affirmed, the presence of hydrogen in graphite determines its combustibility, this can be true only of combined hydrogen, for carbon which has been calcined and made incandescent in an atmosphere of hydrogen is no more combustible than before.—*Technologist; Chron Industr.*, June 1. 1884. C.

Franklin Institute.

[*Proceedings of the Stated Meeting, held September* 17, 1884.]

HALL OF THE INSTITUTE, September 17, 1884.

The President, William P. Tatham, in the Chair.

Present, 42 members. New members elected by the Board of Managers at the meeting of July, 12th, 12; at the meeting of August 13th, 19, and at the meeting of September 10th, 16.

The following action from the August Meeting of the Board was reported:

Resolved, That the Board of Managers recommend the election of M. H. TRESCA, Professor at the Conservatoire des Arts et Metiers, in Paris, as an honorary member of the Franklin Institute.

Prof. Tresca was unanimously elected.

The President announced the death of Professor Robert E. Rogers, for many years a member of the Institute, who had served upon many of its committees, and upon the Board of Managers, and who had acceptably filled the offices of Vice-President and President of the Institute. The President announced that the Board had appointed a committee composed of Mr. J. E. Mitchell, Prof. Edwin J. Houston, and Dr. Isaac Norris, to suitably express the sentiments of the Board.

Mr. Mitchell, seconded by the Secretary, moved that a committee of the Institute be appointed to co-operate with the committee appointed by the Board of Managers to prepare a memorial of the deceased. Carried.

The President, thereupon, appointed Dr. Charles M. Cresson, Dr. G. M. Ward, and Dr. William H. Wahl.

Mr. Hugo Bilgram, seconded by the Secretary, moved that the Committee on Meetings be requested to consider the propriety of providing, at such meetings of the Institute, where discussions are likely to take place, a stenographer, to accurately report the same. Mr. Bilgram intimated that the discussion of papers read before the Institute, as they subsequently appeared in the JOURNAL, did not always give the substance of what was spoken, and that he had found it necessary to make reply to printed statements purporting to be the record of a discussion, which statements he was satisfied were an afterthought, and not made at the meeting.

The Secretary, in seconding the motion, stated that for reports of the discussions and debates, he was at present obliged to depend largely upon the courtesy of the participants to obtain abstracts of their remarks for the JOURNAL. He believed that the adoption of the plan proposed by Mr. Bilgram would have many advantages. It would certainly insure greater accuracy in the reports of discussions. The motion was carried.

Adjourned.

WILLIAM H. WAHL, *Secretary*.

LIST OF BOOKS ADDED TO THE LIBRARY FROM JANUARY TO JUNE, 1884.

Agricultural College of Pennsylvania. Memorial. Feb. 24, 1865.
From the College.

Agricultural College of Pennsylvania. Statements made by Dr. E Pugh to Judiciary Committee. Harrisburg From the College.

Agriculture. Commissioner of. Report for 1883. Washington.
From the Agricultural Department.

Alabama. Geological Survey. Reports of progress for 1875–78 and 1881–82. Montgomery. 1876–79 and 1883. From E. A. Smith State Geologist.

Almanac for 1884. From the *Public Record.*

Almanac for 1884. From the *Philadelphia Inquirer.*

Almanacque Nautico para 1871–77. Cadiz, 1869–76.
From Cecilio Pujazon, Director of Naval Observatory, San Fernando.

Amarican Academy of Medicine. Annual Addresses. 1879–83.
From the Academy.

American Bar Association. Reports of Second to Sixth Annual Meetings. Philadelphia 1879-1883. From the Association.

American Bell Telephone Company, *et. al. vs* The People's Telephone Company, *et al.* From the Company.

American Exchange and Review for 1864–70.
From the Editor, Mr. Fowler.

American Institute of Mining Engineers. Vol. II, 1882–3.
From the Institute.

American Journal of Fabrics and Knit Goods Manufacturer. Vols 3 and 4. 1883–84. From the Publisher.

American Railway Master Mechanics' Association. Report of Proceedings of 16th Annual Convention. Chicago, June, 1883.
From the Association.

American Society of Civil Engineers. Constitution, By-Laws and List of Members. 1884 From the Society.

American Society of Mechanical Engineers. List of Members. January 1, 1884. From the Society.

American Society of Mechanical Engineers. Transactions. Vol. 4, 1883.
From the Society.

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(To be concluded.)

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THE WAVE THEORY OF LIGHT.

By SIR WILLIAM THOMSON, F.R.S., LL.D., Etc.

[A Lecture delivered at the Academy of Music, Philadelphia, under the auspices of the Franklin Institute, September 29, 1884.]

MR. WILLIAM P. TATHAM, President of the Franklin Institute, introduced the lecturer, who spoke as follows:

SIR WILLIAM THOMSON.—Mr. President, Ladies and Gentlemen: The subject upon which I am to speak to you this evening is happily for me not new in Philadelphia. The beautiful lectures on light which were given several years ago by President Morton, of the Stevens' Institute, and the succession of lectures on the same subject so admirably illustrated by Prof. Tyndall, which many now present have heard, have fully prepared you for anything I can tell you this evening in respect to the wave theory of light.

It is indeed my humble part to bring before you some mathematical and dynamical details of this great theory. I cannot have the pleasure of illustrating them to you by anything comparable with the splendid and instructive experiments which many of you have already seen. It is satisfactory to me to know that so many of you, now present, are so thoroughly prepared to understand anything I can say, that those who have seen the experiments will not feel their absence at this time. At the same time I wish to make them intelligible to those who have not had the advantages to be gained by a systematic course of lectures. I must say in the first place, without further preface, as time

is short and the subject is long, simply that sound and light are both due to vibrations propagated in the manner of waves; and I shall endeavor in the first place to define the manner of propagation and mode of motion that constitute those two subjects of our senses, the sense of sound and the sense of light.

Each is due to vibrations. The vibrations of light differ widely from the vibrations of sound. Something that I can tell you more easily than anything in the way of dynamics or mathematics respecting the two classes of vibrations is, that there is a great difference in the frequency of the vibrations of light when compared with the frequency of the vibrations of sound. The term "frequency" applied to vibrations is a convenient term, applied by Lord Rayleigh in his book on sound to a definite number of full vibrations of a vibrating body per unit of time. Consider, then, in respect to sound, the frequency of the vibrations of notes, which you all know in music represented by letters, and by the syllables for singing, the do, re, mi, etc. The notes of the modern scale correspond to different frequencies of vibrations. A certain note and the octave above it correspond to a certain number of vibrations per second and double that number.

I may explain in the first place conveniently the note called "C"; I mean the middle "C"; I believe it is the C of the tenor voice, that most nearly approaches the tones used in speaking. That note corresponds to two hundred and fifty-six full vibrations per second, two hundred and fifty-six times to and fro per second of time.

Think of one vibration per second of time. The seconds pendulum of the clock performs one vibration in two seconds, or a half vibration in one direction per second. Take a ten-inch pendulum of a drawing-room clock, which vibrates twice as fast as the pendulum of an ordinary eight-day clock, and it gives a vibration of one per second, a full period of one per second to and fro. Now think of three vibrations per second. I can move my hand three times per second easily, and by a violent effort I can move it to and fro five times per second. With four times as great force, if I could apply it, I could move it twice five times per second.

Let us think, then, of an exceedingly muscular arm that would cause it to vibrate ten times per second, that is, ten times to the left and ten times to the right. Think of twice ten times, that is, twenty times per second, which would require four times as much force; three times ten, or thirty times a second, would require nine times as much

force. If a person were nine times as strong as the most muscular arm can be, he could vibrate his hand to and fro thirty times per second, and without any other musical instrument could make a musical note by the movement of his hand which would correspond to one of the pedal notes of an organ.

If you want to know the length of a pedal pipe, you can calculate it in this way. There are some numbers you must remember, and one of them is this. You, in this country, are subjected to the British insularity in weights and measures; you use the foot and inch and yard. I am obliged to use that system, but I apologize to you for doing so, because it is so inconvenient, and I hope all Americans will do everything in their power to introduce the French metrical system. I hope the evil action performed by an English minister whose name I need not mention, because I do not wish to throw obloquy on any one, may be remedied. He abrogated a useful rule, which for a short time was followed and which I hope will soon be again enjoined, that the French metrical system be taught in all our national schools. I do not know how it is in America. The school system seems to be very admirable, and I hope the teaching of the metrical system will not be let slip in the American schools any more than the use of the globes.

I say this seriously. I do not think any one knows how seriously I speak of it. I look upon our English system as a wickedly brain-destroying piece of bondage under which we suffer. The reason why we continue to use it is the imaginary difficulty of making a change and nothing else; but I do not think in America that any such difficulty should stand in the way of adopting so splendidly useful a reform.

I know the velocity of sound in feet per second. If I remember rightly, it is 1,089 feet per second in dry air at the freezing point, and 1,115 feet per second in air of what we call moderate temperature, 59 or 60 degrees—(I do not know whether that temperature is ever attained in Philadelphia or not; I have had no experience of it, but people tell me it is sometimes 59 or 60 degrees in Philadelphia, and I believe them)—in round numbers let us call it 1,000 feet per second. Sometimes we call it a thousand musical feet per second, it saves trouble in calculating the length of organ pipes; the time of vibration in an organ pipe is the time it takes a vibration to run from one end to the other and back. In an organ pipe 500 feet long the period would be

one per second; in an organ pipe ten feet long the period would be 50 per second; in an organ pipe twenty feet long the period would be 25 per second at the same rate. Thus 25 per second, and 50 per second of frequencies correspond to the periods of organ pipes of 20 feet and 10 feet.

The period of vibration of an organ pipe, open at both ends, is approximately the time it takes sound to travel from one end to the other and back. You remember that the velocity in dry air in a pipe 10 feet long is a little more than 50 periods per second; going up to 256 periods per second, the vibrations correspond to those of a pipe two feet long. Let us take 512 periods per second; that corresponds to a pipe about a foot long. In a flute, open at both ends, the holes are so arranged that the length of the sound-wave is about one foot, for one of the chief "open notes." Higher musical notes correspond to greater and greater frequency of vibration, viz., 1,000, 2,000, 4,000 vibrations per second; 4,000 vibrations per second correspond to a piccolo flute of exceedingly small length; it would be but one and a half inches long. Think of a note from a little dog-call, or other whistle, one and a half inches long, open at both ends, or from a little key having a tube three quarters of an inch long, closed at one end; you will then have 4,000 vibrations per second.

A wave length of sound is the distance traversed in the period of vibration. I will illustrate what the vibrations of sound are by this condensation travelling along our picture on the screen. Alternate condensations and rarefactions of the air are made continuously by a sounding body. When I pass my hand vigorously in one direction, the air before it becomes dense, and the air on the other side becomes rarified. When I move it in the other direction these things become reversed; there is a spreading out of condensation from the place where my hand moves in one direction and then in the reverse. Each condensation is succeeded by a rarefaction. Rarefaction succeeds condensation at an interval of one-half what we call "wave lengths." Condensation succeeds condensation at the full interval of what we call wave lengths.

We have here these luminous particles on this scale,* representing portions of the air close together, dense; a little higher up, portions of

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* Alluding to a moving diagram of wave motion of sound produced by a working slide for lantern projection.

air less dense. I now slowly turn the handle of the apparatus in the lantern, and you see the luminous sectors showing condensation traveling slowly upwards on the screen; now you have another condensation; making one wave length.

This picture or chart represents a wave length of four feet. It represents a wave of sound four feet long. The fourth part of a thousand is 250. What we see now of the actual scale represents the lower note C of the tenor voice. The air from the mouth of a singer is alternately condensed and rarefied just as you see here.

But that process shoots forward at the rate of one thousand feet per second; the exact period of the motion is 256 vibrations per second for the actual case before you. Follow one particle of the air forming part of a sound wave, as represented by these moving spots of light on the screen; now it goes down, then another portion goes down rapidly; now it stops going down; now it begins to go up; now it goes down and up again.

As the maximum of condensation is approached it is going up with diminishing maximum velocity. The maximum of rarefaction has now reached it, and the particle stops going up and begins to move down. When it is of mean density the particles are moving with maximum velocity, one way or the other. You can easily follow these motions, and you will see that each particle moves to and fro and the thing that we call *condensation* travels along.

I shall show the distinction between these vibrations and the vibrations of light. Here is the fixed appearance of the particles when displaced but not in motion. You can imagine particles of something, the thing whose motion constitutes light. This thing we call the luminiferous ether. That is the only substance we are confident of in dynamics. One thing we are sure of, and that is the reality and substantiality of the luminiferous ether. This instrument is merely a method of giving motion to a diagram designed for the purpose of illustrating wave motion of light. I will show you the same thing in a fixed diagram, but this arrangement shows the mode of motion.

Now follow the motion of each particle. This represents a particle of the luminiferous ether, moving at the greatest speed when it is at the middle position.

You see the two modes of vibration,* sound and light now moving

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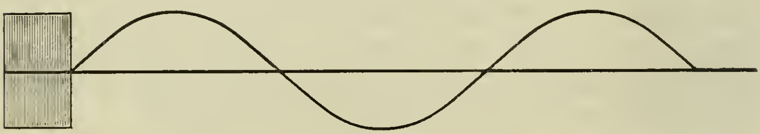
* Showing two moving diagrams, simultaneously, on the screen, one depicting a wave motion of light, the other a sound vibration.

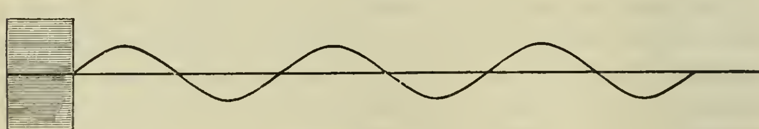
together. The travelling of the wave of condensation and rarefaction, and the travelling of the wave of transverse displacement. Note the direction of propagation. Here it is from your left to your right, as you look at it. Look at the motion when made faster. We have now the direction reversed. The propagation of the wave is from right to left, again the propagation of the wave is from left to right; each particle moves perpendicularly to the line of propagation.

I have given you an illustration of the vibration of sound waves, but I must tell you that the movement illustrating the condensation and rarefaction represented in that moving diagram are necessarily very much exaggerated, to let the motion be perceptible, whereas the greatest condensation in actual sound motion is not more than one or two per cent. or a small fraction of a per cent. Except that the amount of condensation was exaggerated in the diagram for sound, you have a correct representation of what actually takes place in the low note C.

On the other hand, in the moving diagram representing light waves what had we? We had a great exaggeration of the inclination of the

Waves of Red Light.





Waves of Violet Light.

line of particles. You must first imagine a line of particles in a straight line, and then you must imagine them disturbed into a wave curve, the shape of the curve corresponding to the disturbance. Having seen what the propagation of the wave is, look at this diagram and then look at that one. This, in light, corresponds to the different sounds I spoke of at first. The wave length of light is the distance from crest to crest of the wave, or from hollow to hollow. I speak of crests and hollows, because we have a diagram of ups and downs as the diagram is placed.

Here, then, you have a wave length.* In this lower diagram you

* Exhibiting a large drawing, or chart, representing a red and a violet wave of light.

have the wave length of violet light. It is but one-half the length of the upper wave of red light; the period of vibration is but half as long. Now, on an enormous scale, exaggerated not only as to slope, but immensely magnified as to wave-length, we have an illustration of the waves of light. The drawing marked "red" corresponds to red light, and this lower diagram corresponds to violet light. The upper curve really corresponds to something a little below the red ray of light in the spectrum, and the lower curve to something beyond the violet light. The variation in length between the most extreme rays is in the proportion of four and a half of red to eight of the violet, instead of four and eight; the red waves are nearly as one to two of the violet.

To make a comparison between the number of vibrations for each wave of sound and the number of vibrations constituting light waves, I may say that 30 vibrations per second is about the smallest number which will produce a musical sound; 50 per second gives one of the grave pedal notes of an organ, 100 or 200 per second give the low notes of the bass voice, higher notes with 250 per second, 300 per second, 1,000, 4,000, up to 8,000 per second give about the shrillest notes audible to the human ear.

Instead of the numbers, which we have, say in the most commonly used part of the musical scale, *i.e.*, from 200 or 300 to 600 or 700 per second, we have millions and millions of vibrations per second in light waves; that is to say, 400 million million per second, instead of 400 per second. That number of vibrations is performed when we have red light produced.

An exhibition of red light travelling through space from the remotest star is due to the propagation by waves or vibrations, in which each individual particle of the transmitting medium vibrates to and fro 400 million million times in a second.

Some people say they cannot understand a million million. Those people cannot understand that twice two makes four. That is the way I put it to people who talk to me about the incomprehensibility of such large numbers. I say *finitude* is incomprehensible, the infinite in the universe *is* comprehensible. Now apply a little logic to this. Is the negation of infinitude incomprehensible? What would you think of a universe in which you could travel one, ten, or a thousand miles, or even to California, and then find it come to an end? Can you suppose an end of matter, or an end of space? The idea is incom-

prehensible. Even if you were to go millions and millions of miles the idea of coming to an end is incomprehensible.

You can understand one thousand per second as easily as you can understand one per second. You can go from one to ten, and ten times ten and then to a thousand without taxing your understanding, and then you can go on to a thousand million and a million million. You can all understand it.

Now 400 million million vibrations per second is the kind of thing that exists as a factor in the illumination by red light. Violet light, after what we have seen and have illustrated by that curve, I need not tell you corresponds to vibrations of 800 million million per second. There are recognizable qualities of light caused by vibrations of much greater frequency and much less frequency than this. You may imagine vibrations having about twice the frequency of violet light and one-fifteenth the frequency of red light and still you do not pass the limit of the range of continuous phenomena only a part of which constitutes *visible* light.

Everybody knows the "photographer's light" and has heard of *invisible* light producing visible effects upon the chemically prepared plate in the camera. Speaking in round numbers, I may say that, in going up to about twice the frequency I have mentioned for violet light you have gone to the extreme end of the range of known light of the highest rates of vibration; I mean to say that you have reached the greatest frequency that has yet been observed.

When you go below visible red light what have you? We have something we do not see with the eye, something that the ordinary photographer does not bring out on his photographically sensitive plates. It is light, but we do not see it. It is something so closely continuous with light visible, that we may define it by the name of invisible light. It is commonly called radiant heat; invisible radiant heat. Perhaps, in this thorny path of logic, with hard words flying in our faces, the least troublesome way of speaking of it is to call it radiant heat. The heat effect you experience when you go near a bright, hot coal fire, or a hot steam boiler; or when you go near, but not over, a set of hot water pipes used for heating a house; the thing we perceive in our face and hands when we go near a boiling pot and hold the hand on a level with it, is radiant heat; the heat of the hands and face caused by a hot fire, or a hot kettle when held under the kettle, is also radiant heat.

You might readily make the experiment with an earthen teapot; it radiates heat better than polished silver. Hold your hands below and you perceive a sense of heat; above the teapot you get more heat; either way you perceive heat. If held over the teapot you readily understand that there is a little current of air rising. If you put your hand under the teapot you get cold air; the upper side of your hand is heated by radiation while the lower side is fanned and is actually cooled by virtue of the heated kettle above it.

That perception by the sense of heat, is the perception of something actually continuous with light. We have knowledge of rays of radiant heat perceptible down to (in round numbers) about four times the wave length, or one-fourth the period of visible, or red light. Let us take red light at 400 million million vibrations per second; then the lowest radiant heat, as yet investigated, is about 100 million million per second in the way of frequency of vibration.

I had hoped to be able to give you a lower figure. Prof. Langley has made splendid experiments on the top of Mount Whitney, at the height of 15,000 feet above the sea level, with his "Bolometer," and has made actual measurements of the wave lengths of radiant heat down to exceedingly low figures. I will read you one of the figures; I have not got it by heart yet, because I am expecting more from him.* I learned a year and a half ago that the lowest radiant heat observed by the diffraction method of Prof. Langley corresponded to 28 one hundred thousandths of a centimetre for wave length, 28 as compared with red light, which is 7·3; or nearly four-fold. Thus wave lengths of four times the amplitude, or one-fourth the frequency per second of red light have been experimented on by Prof. Langley and recognized as radiant heat.

Photographic, or actinic light, as far as our knowledge extends at present, takes us to a little less than one-half the wave length of violet light. You will thus see that while our acquaintance with wave motion below the red extends down to one-quarter of the slowest rate which affects the eye, our knowledge of vibrations at the other

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* Since my lecture I have heard from Prof. Langley that he has measured the refrangibility by a rock salt prism, and inferred the wave length of heat rays from a "Leslie cube" (a metal vessel of hot water radiating from a blackened side). The greatest wave length he has thus found is one one-thousandth of a centimetre, which is seventeen times that of sodium light. The corresponding period as about thirty million million to the second.

W. T.

end of the scale only comprehends those having twice the frequency of violet light. In round numbers we have 4 octaves of light, corresponding to 4 octaves of sound in music. In music the octave has a range to a note of double frequency. In light we have one octave of visible light, one octave above the visible range and two octaves below the visible range. We have 100 per second, 200 per second, 400 per second (million million understood) for invisible radiant heat, 800 per second for visible light, and 1,600 per second for invisible light.

One thing in common to the whole is the heat effect. It is extremely small in moonlight, so small that nobody until recently knew there was any heat in the moon's rays. Herschel thought it was perceptible in our atmosphere by noticing that it dissolved away very light clouds, an effect which seemed to show in full moonlight more than when we have less than full moon. Herschel, however, pointed this out as doubtful, but now, instead of its being a doubtful question, we have Prof. Langley giving as a fact that the light from the moon drives the indicator of his sensitive instrument clear across the scale and with a comparatively prodigious heating effect!

I must tell you that if any of you want to experiment with the heat of moonlight you must compare the heat with whatever comes within the influence of the moon's rays only. This is a very necessary precaution; if, for instance, you should take your Bolometer or other heat detector from a comparatively warm room into the night air, you would obtain an indication of a fall in temperature owing to this change. You must be sure that your apparatus is in thermal equilibrium with the surrounding air, then take your burning-glass, and first point it to the moon and then to space in the sky beside the moon; you thus get a differential measurement in which you compare the radiation of the moon with the radiation of the sky. You will then see that the moon has a distinctly heating effect.

To continue our study of visible light, that is, undulations extending

The Solar Spectrum.

from red to violet in the spectrum (which I am going to show you presently), I would first point out on this chart that in the section from letter "*A*" to letter "*D*" we have visual effect and heating effect only;

but no ordinary chemical or photographic effect. Photographers can leave their usual sensitive, chemically prepared plates exposed to yellow light and red light without experiencing any sensible effect; but when you get toward the blue end of the spectrum the photographic effect begins to tell, more and more as you get toward the violet end. When you get beyond the violet there is the invisible light known chiefly by its chemical action. From yellow to violet we have visual effect, heating effect and chemical effect, all three; above the violet only chemical and heating effects and so little of the heating effect that it is scarcely perceptible.

The prismatic spectrum is Newton's discovery of the composition of white light. White light consists of every variety of color from red to violet. Here, now, we have Newton's prismatic spectrum produced by a prism. I will illustrate a little in regard to the nature of color by putting something before the light which is like colored glass; it is colored gelatin. I will put in a plate of red gelatin which is carefully prepared of chemical materials and see what that will do. Of all the light passing to it from violet to red it only lets through the red and orange, giving a mixed reddish color.

Here is another plate of green gelatin. The green absorbs all the red, giving only green. Here is another plate absorbing something from each portion of the spectrum, taking away a great deal of the violet and giving a yellow or orange appearance to the light. Here is another absorbing out the green, leaving red, orange, and a very little faint green, and absorbing out all the violet.

When the spectrum is very carefully produced, far more perfectly than Newton knew how to show it, we have a homogeneous spectrum. It must be noticed that Newton did not understand what we call a homogeneous spectrum; he did not produce it, and does not point out in his writings the conditions for producing it. With an exceedingly fine line of light we can bring it out as in sunlight, like this upper picture, red, orange, yellow, green, blue, indigo and violet according to Newton's nomenclature. Newton never used a narrow beam of light, and so could not have had a homogeneous spectrum.

This is a diagram painted on glass and showing the colors as we know them. It would take two or three hours if I were to explain the subject of spectrum analysis to-night. We must tear ourselves away from it. I will just read out to you the wave lengths corresponding to the different positions in the sun's spectrum of certain dark

lines commonly called "Fraunhofer's lines." I will take as a unit the one-hundredth thousandth of a centimetre. A centimetre is ·4 of an inch; it is a rather small half an inch. I take the thousandth of a centimetre and the hundredth of that as a unit. At the red end of the spectrum the light in the neighborhood of that black line "*A*" has for its wave length 7·6; "*B*" has 6·87; "*D*" has 5·89; the "frequency" for "*A*" is 3·9 times 100 million million; the frequency of "*D*" light is 5·1 times 100 million million per second.

Now what force is concerned in those vibrations as compared with sound at the rate of 400 vibrations per second; suppose for a moment the same matter was to move to and fro through the same range but 400 million million times per second. The force required is as the square of the number expressing the frequency. Double frequency would require quadruple force for the vibration of the same body. Suppose I vibrate my hand again, as I did before. If I move it once per second a moderate force is required; for it to vibrate ten times per second 100 times as much force is required; for 400 vibrations per second 160,000 times as much force.

If I move my hand once per second through a space of quarter of an inch a very small force is required; it would require very considerable force to move it ten times a second, even through so small a range; but think of the force required to move a tuning fork 400 times a second; compare that with the force required for a motion of 400 million million times a second. If the mass moved is the same, and the range of motion is the same, then the force would be one million million million million times as great as the force required to move the prongs of the tuning fork. It is as easy to understand that number as any number like 2, 3, or 4.

Consider gravely what that number means and what we are to infer from it. What force is there in space between my eye and that light? What forces are there in space between our eyes and the sun and our eyes and the remotest visible star? There is matter and there is motion, but what magnitude of force may there be?

I move through this "luminiferous ether" as if it were nothing. But were there vibrations with such frequency in a medium of steel or brass, they would be measured by millions and millions and millions of tons action on a square inch of matter. There are no such forces in our air. Comets make a disturbance in the air and perhaps the luminiferous ether is split up by the motion of a comet through it. So

when we explain the nature of electricity, we explain it by a motion of the luminiferous ether. We cannot say that it is electricity. What can this luminiferous ether be? It is something that the planets move through with the greatest ease. It permeates our air; it is nearly in the same condition, so far as our means of judging are concerned, in our air and in the inter-planetary space. The air disturbs it but little; you may reduce air by air pumps to the hundredth thousandth of its density, and you make little effect in the transmission of light through it. The luminiferous ether is an elastic solid. The nearest analogy I can give you is this jelly which you see.* The nearest analogy to the waves of light is the motion, which you can imagine, of this elastic jelly, with a ball of wood floating in the middle of it. Look there, when with my hand I vibrate the little red ball up and down, or when I turn it quickly round the vertical diameter, alternately in opposite directions;—that is the nearest representation I can give you of the vibrations of luminiferous ether.

Another illustration is Scottish shoemaker's wax or Burgundy pitch, but I know Scottish shoemaker's wax better. It is heavier than water and absolutely answers my purpose. I take a large slab of the wax, place it in a glass jar filled with water, place a number of corks on the lower side and bullets on the upper side. It is brittle like the Trinidad pitch or Burgundy pitch which I have in my hand. You can see how hard it is, but if left to itself it flows like a fluid. The shoemaker's wax breaks with a brittle fracture, but it is viscous and gradually yields.

What we know of the luminiferous ether is that it has the rigidity of a solid and gradually yields. Whether or not it is brittle and cracks we cannot yet tell, but I believe the discoveries in electricity and the motions of comets and the marvelous spurts of light from them, tend to show cracks in the luminiferous ether—show a correspondence between the electric flash and the aurora borealis and cracks in the luminiferous ether. Do not take this as an assertion, it is hardly more than a vague scientific dream: but you may regard the existence of the luminiferous ether as a reality of science, that is, we have an all-pervading medium, an elastic solid, with a great degree of rigidity; its rigidity is so prodigious in proportion to its density that the vibra-

* Exhibiting a large bowl of clear jelly with a small red wooden ball imbedded in the surface near the centre.

tions of light in it have the frequencies I have mentioned, with the wave lengths I have mentioned.

The fundamental question as to whether or not luminiferous ether has gravity has not been answered. We have no knowledge that the luminiferous ether is attracted by gravity; it is sometimes called imponderable because some people vainly imagine that it has no weight. I call it matter with the same kind of rigidity that this elastic jelly has.

Here are two tourmalines; if you look through them toward the light you see the white light all round, *i. e.* they are transparent. If I turn round one of these tourmalines the light is extinguished, it is absolutely black, as though the tourmalines were opaque. This is an illustration of what is called polarization of light. I cannot speak to you about qualities of light without speaking of the polarization of light. I want to show you a most beautiful effect of polarizing light, before illustrating a little further by means of this large mechanical illustration which you have in the bowl of jelly. Now I put in the lantern another instrúment called a "Nicol prism." What you saw first were two plates of the crystal tourmaline which came from Brazil, I believe, having the property of letting light pass when both plates are placed in one particular direction as regards their axes of crystallization, and extinguishing it when it passes through the first plate held in another direction. We have now an instrument which also gives rays of polarized light. A Nicol prism is a piece of Iceland spar, cut in two and turned, one part relatively to the other in a very ingenious way, and put together again and cemented into one by Canada balsam. The Nicol prism takes advantage of the property which the spar has of double refraction, and produces the phenomenon which I now show you.

I turn one prism round in a certain direction and you get light, a maximum of light. I turn it through a right angle and you get blackness. I turn it one quarter round again and get maximum light; one quarter more, maximum blackness; one quarter more and bright light. We rarely have such a grand specimen of a Nicol prism as this.

There is another way of producing polarized light. I stand before that light and look at its reflection in a plate of glass on the table through one of the Nicol prisms, which I turn round, so. Now I must incline that piece of glass at a particular angle, rather more than forty-five degrees; I find a particular angle in which, if I look at it

and then turn the prism round in the hand, the effect is absolutely to extinguish the light in one position and to give it maximum brightness in another position. I use the term "absolute" somewhat rashly. It is only a reduction to a very small quantity of light, not an absolute annulment as we have in the case of the two Nicol prisms used conjointly. Those of you who have never heard of this before would not know what I am talking about. As to the mechanics of the thing it could only be explained to you by a course of lectures in physical optics. The thing is this, vibrations of light must be in a definite direction relatively to the line in which the light travels.

Look at this diagram, the light goes from left to right; we have vibrations perpendicular to the line of transmission. There is a line up and down which is the line of vibration. Imagine here a source of light, violet light, and here in front of it is the line of propagation. Sound vibrations are to and fro; this is transverse to the line of propagation. Here is another, perpendicular to the diagram, still following the law of transverse vibration; here is another circular vibration. Imagine a long rope, you whirl one end of it and you send a screw like motion running along; you can get the circular motion in one direction or in the opposite.

Plane polarized light is light with the vibrations all in a single plane, perpendicular to the plane through the ray which is technically called the "plane of polarization." Circular polarized light consists of undulations of luminiferous ether having a circular motion. Elliptically polarized light is something between the two, not in a straight line, and not in a circular line; the course of vibration is an ellipse. Polarized light is light that performs its motions continually in one mode or direction. If in a straight line it is plane polarized; if in a circular direction it is circularly polarized light; when elliptical it is elliptically polarized light.

With Iceland spar, one unpolarized ray of light divides on entering it into two rays of polarized light, by reason of its power of double refraction, and the vibrations are perpendicular to one another in the two emerging rays. Light is always polarized when it is reflected from a plate of unsilvered glass, or water, at a certain definite angle of fifty-six degrees for glass, fifty-two degrees for water, the angle being reckoned in each case from a perpendicular to the surface. The angle for water is the angle whose tangent is 1·4. I wish you to look at the polarization with your own eyes. Light from glass at fifty-six

degrees and from water at fifty-two degrees goes away vibrating perpendicularly to the plane of incidence and plane of reflection.

We can distinguish it without the aid of an instrument. There is a phenomenon well known in physical optics as "Haidinger's Brushes." The discoverer is well known in Philadelphia as a mineralogist, and the phenomenon I speak of goes by his name. Look at the sky in a direction of ninety degrees from the sun, and you will see a yellow and blue cross, with the yellow toward the sun, and from the sun, spreading out like two foxes tails with blue between, and then two red brushes in the space at right angles to the blue. If you do not see it, it is because your eyes are not sensitive enough, but a little training will give them the needed sensitiveness.

If you cannot see it in this way try another method. Look into a pail of water with a black bottom; or take a clear glass dish of water, rest it on a black cloth and look down at the surface of the water on a day with a white cloudy sky (if there is such a thing ever to be seen in Philadelphia). You will see the white sky reflected in the basin of water at an angle of about fifty degrees. Look at it with the head tipped to one side and then again with the head tipped to the other side, keeping your eyes on the water, and you will see Haidinger's brushes. Do not do it fast or you will make yourself giddy. The explanation of this is the refreshing of the sensibility of the retina. The Haidinger's brush is always there, but you do not see it because your eye is not sensitive enough. After once seeing it you always see it; it does not thrust itself inconveniently before you when you do not want to see it. You can readily see it in a piece of glass with dark cloth below it, or, in a basin of water.

I am going to conclude by telling you how we know the wave lengths of light and how we know the frequency of the vibrations. We shall actually make a measurement of the wave length of the yellow light. I am going to show you the diffraction spectrum.

You see on the screen,* on each side of a central white bar of light, a set of bars of light variegated colors, the first one, on each side, showing blue or indigo color, about four inches from the central white bar and red about four inches farther, with vivid green between the blue and the red. That effect is produced by a grating with 400 lines

* Showing the chromatic bands thrown upon the screen from a diffraction grating.

to the centimetre, engraved on glass, which I now hold in my hand. The next grating has 3,000 lines on a Paris inch. You see the central space and on each side a large number of spectrums, blue at one end and red at the other. The fact that, in the first spectrum, red is about twice as far from the centre as the blue, proves that a wave length of red light is double that of blue light.

I will now show you the operation of measuring the length of a wave of sodium light, that is a light like that marked "*D*" on the spectrum, a light produced by a spirit lamp with salt in it. The sodium vapor is heated up to several thousand degrees, when it becomes self luminous and gives such a light as we get by throwing salt upon a spirit lamp in the game of snap dragon.

I hold in my hand a beautiful grating of glass silvered by Liebig's process with metallic silver, a grating with 6,480 lines to the inch, belonging to my friend Prof. Barker, which he has kindly brought here for us this evening. You will see the brilliancy of color as I turn the light reflected from the grating toward you and pass the beam round the room. You have now seen directly with your own eyes these brilliant colors reflected from the grating, and you have also seen them thrown upon the screen from a grating placed in the lantern. With a grating of 17,000 lines—a much greater number of lines per inch than the other—you will see how much further from the central bright space the first spectrum is; how much more this grating changes the direction or diffraction of the beam of light. Here is the centre of the grating, and there is the first spectrum. You will note that the violet light is least diffracted and the red light is most diffracted. This diffraction of light first proved to us definitely the reality of the undulatory theory of light.

You ask why does not light go 'round a corner as sound does. Light does go round a corner in these diffraction spectrums; it is shown going round a corner, it passes through these bars and is turned round an angle of thirty degrees. Light going round a corner by instruments adapted to show the result, and to measure the angles at which it is turned, is called the diffraction of light.

I can show you an instrument which will measure the wave lengths of light. Without proving the formula, let me tell it to you. A spirit lamp with salt sprinkled on the wick gives very nearly homogeneous light, that is to say, light all of one wave length, or all of the same period. I have a little grating which I take in my hand. I look

through this grating and see that candle before me. Close behind it you see a blackened slip of wood with two white marks on it ten inches asunder. The line on which they are marked is placed perpendicular to the line at which I shall go from it. When I look at this salted spirit lamp I see a series of spectrums of yellow light. As I am somewhat short-sighted I am making my eye see with this eyeglass and the natural lenses of the eye what a long-sighted person would make out without an eyeglass. On that screen you saw a succession of spectrums. I now look direct at the candle and what do I see? I see a succession of five or six brilliantly colored spectrums on each side of the candle. But when I look at the salted spirit lamp, now I see ten spectrums on one side and ten on the other, each of which is a monochromatic band of light.

I will measure the wave lengths of light thus. I walk away to a considerable distance and look at the candle and marks. I see a set of spectrums. The first white line is exactly behind the candle. I want the first spectrum to the right of that white line to fall exactly on the other white line, which is ten inches from the first. As I walk away from it I see it is now very near it; it is now on it. Now the distance from my eye is to be measured and the problem is again to reduce feet to inches. The distance from the spectrum of the flame to my eye is thirty-four feet nine inches. Mr. President, how many inches is that? 417 inches, in round numbers 420 inches. Then we have the proportion, as 420 is to 10 so is the length from bar to bar of the grating to the wave length of sodium light. That is to say as forty-two is to one. The distance from bar to bar is the four hundredth of a centimetre: therefore the 42d part of the four hundredth of a centimetre is the required wave length, or the 16,800th of a centimetre is the wave length according to our simple, and easy and hasty experiment. The true wave length of sodium light, according to the most accurate measurement, is about a 17,000th of a centimetre, which differs by scarcely more than one per cent. from our result!

The only apparatus you see is this little grating; it is a piece of glass with four-tenths of an inch ruled with 400 fine lines. Any of you who will take the trouble to buy one may measure the wave lengths of a candle flame himself. I hope some of you will be induced to make the experiment for yourselves.

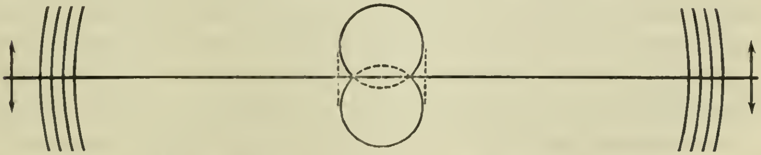
If I put salt on the flame of a spirit lamp, what do I see through this grating? I see merely a sharply defined yellow light, constitut-

ing the spectrum of vaporized sodium, while from the candle flame I see an exquisitely colored spectrum, far more beautiful than I showed you on the screen. I see in fact a series of spectrums on the two sides with the blue toward the candle flame and the red further out. I cannot get one definite thing to measure from in the spectrum from the candle flame as I can with the flame of a spirit lamp with the salt thrown on it, which gives as I have said a simple yellow light. The highest blue light I see in the candle flame is now exactly on the line. Now measure to my eye, it is forty-four feet four inches, or 532 inches. The length of this wave then is the 532d part of the four hundredth of a centimetre which would be the 21,280th of a centimetre, say the 21,000th of a centimetre. Then measure for the red and you would find something like the 11,000th for the lowest of the red light.

Lastly, how do we know the frequency of vibration?

Why, by the velocity of light. How do we know that? We know it in a number of different ways, which I cannot explain now because time forbids. Take the velocity of light. It is 187,000 British statute miles per second. But it is much better to take a kilometre for the unit. That is about six-tenths of a mile. The velocity is very accurately 300,000 kilometers per second; that is 30,000,000,000 centimetres per second. Take the wave length as the 17,000th of a centimetre and you find the frequency of the sodium light to be 510 million million per second. There, then, you find a calculation of the frequency from a simple observation which you can all make for yourselves.

Lastly, I must tell you about the color of the blue sky which was illustrated by the spherule imbedded in an elastic solid. I want to



Vibrating Spherule Imbedded in an Elastic Solid.

explain to you in two minutes the mode of vibrations. Take the simplest plane-polarized light. Here is a spherule which is producing it in an elastic solid. Imagine the solid to extend miles horizontally and miles down, and imagine this spherule to vibrate up and down. It is quite clear that it will make transverse vibrations similarly in

all horizontal directions. The plane of polarization is defined as a plane perpendicular to the line of vibration. Thus, light produced by a molecule vibrating up and down, as this red globe in the jelly before you, is polarized in a horizontal plane because the vibrations are vertical.

Here is another mode of vibrations. Let me twist this spherule in the jelly as I am doing it, and that will produce vibrations, also spreading out equally in all horizontal directions. When I twist this globe round it draws the jelly round with it; twist it rapidly back and the jelly flies back. By the inertia of the jelly the vibrations spread in all directions and the lines of vibration are horizontal all through the jelly. Everywhere, miles away that solid is placed in vibration. You do not see it, but you must understand that they are there. If it flies back it makes vibration, and we have waves of horizontal vibrations traveling out in all directions from the exciting molecule.

I am now causing the red globe to vibrate to and fro horizontally. That will cause vibrations to be produced which will be parallel to the line of motion at all places of the plane perpendicular to the range of the exciting molecule. What makes the blue sky? These are exactly the motions that make the blue light of the sky which is due to spherules in the luminiferous ether, but little modified by the air. Think of the sun near the horizon, think of the light of the sun streaming through and giving you the azure blue and violet overhead. Think first of any one particle of the sun and think of it moving in such a way as to give horizontal and vertical vibrations and what not of circular and elliptic vibrations.

You see the blue sky in high pressure steam blown into the air; you see it in the experiment of Tyndall's blue sky in which a delicate condensation of vapor gives rise to exactly the azure blue of the sky.

Now the motion of the luminiferous ether relatively to the spherule gives rise to the same effect as would an opposite motion impressed upon the spherule quite independently by an independent force. So you may think of the blue color coming from the sky as being produced by to and fro vibrations of matter in the air which vibrates much as this little globe vibrates imbedded in the jelly.

The result in a general way is this: The light coming from the blue sky is polarized in a plane through the sun, but the blue light of the sky is complicated by a great number of circumstances and one of them is this, that the air is illuminated not only by the sun but by the

earth. If we could get the earth covered by a black cloth then we could study the polarized light of the sky with simplicity, which we cannot do now. There are, in nature, reflections from seas and rocks and hills and waters in a indefinitely complicated manner.

Let observers observe the blue sky not only in winter when the earth is covered with snow, but in summer when it is covered with dark green foliage. This will help to unravel the complicated phenomena in question. But the azure blue of the sky is light produced by the reaction on the vibrating ether of little spherules of water, of perhaps a fifty thousandth or a hundred thousandth of a centimetre diameter, or perhaps little motes, or lumps, or crystals of common salt, or particles of dust, or germs of vegetable or animal species wafted about in the air. Now what is the luminiferous ether? It is matter prodigiously less dense than air—millions and millions and millions of times less dense than air. We can form some sort of idea of its limitations. We believe it is a real thing, with great rigidity in comparison with its density, and it may be made to vibrate 400 million million times per second, and yet with such rigidity as not to produce the slightest resistance to any body going through it.

Going back to the illustration of the shoemaker's wax; if a cork will in the course of a year push its way up through a plate of that wax when placed under water, and if a lead bullet will penetrate downwards to the bottom, what is the law of the resistence? It clearly depends on time. The cork slowly in the course of a year works its way up through two inches of that substance; give it one or two thousand years to do it and the resistance will be enormously less; thus the motion of a cork or bullet, at the rate of one inch in 2,000 years, may be compared with that of the earth, moving at the rate of six times ninety-three million miles a year, or nineteen miles per second, through the luminiferous ether; but when we have a thing elastic like jelly and yielding like pitch, surely we have a large and solid ground for our faith in the speculative hypothesis of an elastic luminiferous ether, which constitute, the wave theory of light.

Mr. Tatham—On behalf of the members of the Franklin Institute I desire publicly to return thanks to Professor Sir William Thomson for his instructive lecture.

HARMONIC MOTION IN STELLAR SYSTEMS.

By Pliny Earle Chase, LL.D.

[Read at the Philadelphia Meeting of the American Association for the Advancement of Science.]

The principle of harmonic motion is of "immense use, not only in ordinary kinetics, but in the theories of sound, light, heat, etc." (Thomson and Tait, *Nat. Phil.*, i, sec. 52).

In applying the doctrine of conservation of energy to the nebular hypothesis, the principles of universal gravitation and of æthereal oscillation are simultaneously and mutually operative, in ways which are indicated, as I think, by observable relations among cosmical masses, distances, velocities and orbital periods. Among the evidences of far-reaching kinetic correlation the following seem to be worthy of special consideration:

1. Cosmical masses and æthereal waves are in contact and in relative motion. If the luminiferous æther is subject to gravitation, the waves are under conditions of impact, in which gravity acts constantly towards the cosmical centres. The oscillating time-integral, gt, should therefore have an important kinetic significance. If g represents the greatest gravitating acceleration in our stellar system (g at Sun's surface), and t represents the time required to convert centripetal acceleration into circular-orbital revolution which is synchronous with solar rotation, the time-integral is *the velocity of light* (v_λ).

2. Centrifugal radiation may be considered as opposed by centripetal gravitation. The total cyclical resistance is measured by the same time-integral, $gt = v_\lambda$.

3. According to the electro-magnetic theory of light, the ratio of the electric units may be represented by the same time-integral, $v_e = gt = v_\lambda$.

4. In the plane of luminous undulation, the electrical disturbance parallel to the equatorial tangent and the magnetic disturbance parallel to the axis suggest the application of Coulomb's formula of torsional elasticity:

$$f = \frac{m}{2} = \frac{W}{2} \cdot \frac{\pi^2 a^2 r_0}{gt^2}\,;\ \therefore\ \pi^2 a^2 r_0 = gt^2 = \pi^2 l.$$

In this formula, if r_0 = Sun's semidiameter, we have the same time-integral, $gt = v_\lambda$.

5. Harmonic motions, both simple and cumulative, are indicated by the following equations: $gt_a = \frac{2}{\pi} v_r$; $gt_\beta = v_p$; $gt_\gamma = v_\lambda$. In these equations, t_a = time in which a luminous undulation would traverse Sun's diameter; v_r = equatorial velocity of solar rotation; t_β = time in which solar superficial gravitation would communicate parabolic velocity, or velocity of infinite fall; t_γ = solar cyclical period of half rotation, giving the same time-integral, $gt = v_\lambda$.

6. Various forms of cyclical oscillation, representing the immediate combined control of inertia, attraction and repulsion, may be denoted and co-ordinated by the formula

$$gt^2 = \pi^2 l = \pi^2 L^3 l_0 = M = 2h.$$

In this formula g represents gravitating acceleration; t = cyclical time of a single oscillation (or half time of rotation or of revolution); l = length of linear pendulum, or radius of circular-orbital revolution; L = quotient of minimum radius of free revolution by radius of synchronous constrained nucleal rotation, as explained in a following paragraph; $l_0 = \rho_0$ = radius of solar or stellar nucleus; M = modulus, or height of homogeneous elastic atmosphere which would propagate waves with velocity gt; h = height of fall in time t, or of projection against constant g by projectile velocity gt, or of alternate fall and rise in perpetual elastic rebound with maximum velocity gt.

At Sun's surface, where the collisions of subsiding particles have changed free revolution into constrained rotation, the time-integral gt, as we have seen, $= v_\lambda$.

In free circular orbital revolution, $t \propto r^{\frac{3}{2}}$; but in the spiral paths of particles in an expanding or contracting rotating nucleus, t varies, either exactly or very nearly, as r^2, while $g \propto \frac{1}{r^2}$. Therefore, gt is nearly or quite* constant, and we have reason to believe that the modulus velocity of æthereal oscillation, of nucleal rotation, of electric

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* The most recent investigations of Earth's ellipticity, by Fischer, Listing, Jordan and Clarke, seem, by analogy, to increase the probability that the mean constancy is complete, the æthereal rigidity and strain producing such "flow of solids" and oblateness of interior strata (Thomson and Tait, *op. cit.*, sec. 821) as are required for stability of figure. If there has been the same extent of constancy in the Sun as there appears to have been in the Earth, the solar oblateness has not yet reached $\frac{1}{50000}$.

ratio, of solar torsion and of limiting gravitating acceleration, has been, is and will continue to be, v_λ. This gives the universal equation

$$g = \frac{v_\lambda}{t} \cdot \frac{m}{r^2}$$

for all masses and distances, provided m and r are expressed in terms of Sun's mass and semidiameter.

Laplace (Bowditch's translation, III, vii, §47 [2128²]) shows that the solar "atmosphere can extend no farther than to the orbit of a planet whose periodical revolution is performed in the same time as the sun's rotatory motion about its axis." Faye (*Comptes Rendus*, April 21, 1884, p. 949) traces the indication of this limit to Kant. I designate it, therefore, by ρ_k, Sun's semidiameter being ρ_o, and $L = \rho_k \div \rho_o$. It may be deduced as follows:

Let l = length of a pendulum, measured from the point of suspension to the centre of oscillation; $\frac{3}{2} l$ = total length of a synchronous linear pendulum; $\frac{3}{2} \pi^2 l$ = corresponding radius of constrained rotation $= \rho_o$; $(\frac{3}{2} \pi^2)^{\frac{4}{3}} \rho_o = \rho_k = 36{\cdot}35 \rho_o$.* Combining this result with the British Nautical Almanac estimate of Sun's apparent semidiameter, 961·82″, we find the following values:

$$n = \rho_3 \div \rho_o = 206264{\cdot}806'' \div 961{\cdot}82'' = 214{\cdot}45 \qquad 7$$

$$v_o = \sqrt{g_o \rho_o} = 2\pi\rho_o \times n^{\frac{3}{2}} \div 31558149 = {\cdot}0006252554 \rho_o \qquad 8$$

$$v_\lambda \div v_o = \pi L^{\frac{3}{2}} = 688{\cdot}544 \qquad 9$$

$$v_r = v_o \div L^{\frac{3}{2}} = {\cdot}0000028528 \rho_o \qquad 10$$

$$M = \pi^2 L^3 \rho_o = 474093 \rho_o \qquad 11$$

$$v_\lambda = {\cdot}43056 \rho_o = g_o t \qquad 12$$

$$t = M \div v_\lambda = 1101221 \text{ s.} = 12{\cdot}746 \text{ dys.} \qquad 13$$

$$g_o = v_\lambda \div t = {\cdot}000000390944 \rho_o = v_o^2 \div \rho_o \qquad 14$$

$$t_\lambda = 214{\cdot}45 \rho_o \div v_\lambda = 498{\cdot}123^s \qquad 15$$

$$C_a = 1296000'' \times t_\lambda \div 31558149^s = 20{\cdot}456'' \qquad 16$$

Nyrén's estimate of the constant of aberration (C_a; see *The Observatory*, vi, 365) is 20·492."

Equations 8, 10, 11, 12, 14 contain the factor ρ_o, which could be

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* The nucleal radius $\propto t^{\frac{1}{2}}$, while the atmospheric radius $\propto t^{\frac{2}{3}}$; $\therefore \rho_k \propto \rho_o^{\frac{4}{3}}$.

readily found if we knew the quotient of Sun's mass by Earth's mass, $m_0 \div m_3$. The great ratio of æthereal elasticity to density may be expected to introduce various harmonic relations of mass. For a clue to some of those relations we may reasonably look to gravitating potential and æthereal oscillation. The ordinary expression for gravitating potential represents a *vis viva* in the plane of luminous oscillation, but for thermodynamic reasons we may also look for a radial *vis viva*, dependent upon the central mass and the sum of the squares of gravitating acceleration on all æthereal particles during the mean interval of a luminous wave.

A *vis viva* of this kind would be proportional to the cube of the mass. In the resultant cyclic harmonies which have given stability to our stellar system, there are evidences of direct and reciprocal, æthereal and cosmical, *vis viva* and momentum, as is shown by the following proportions:

$$m_3^3 : m_0 m_3 m_5 :: \mu v_3^2 : \mu v_\lambda^2 \qquad 17$$

$$m_5^3 : m_0 m_3 m_6 :: \mu v_\beta : \mu v_0 \qquad 18$$

$$m_5^3 : m_6^3 :: \mu v_0^2 :: \mu v_k^2 :: \rho_k : \rho_0 \qquad 19$$

$$(m_1 + m_4)^3 : (m_2 + m_3)^3 :: (m_7 + m_8)^3 : m_5^3 :: V_l : V_\mu :: 1 : \pi^6 \qquad 20$$

$$m_2^3 : m_3^3 :: m_2 v_3^2 : m_3 v_2^2 :: t_2 : t_3 \qquad 21$$

$$m_1^3 : m_4^3 :: \mu v_4^2 : \mu v_1^2 :: \rho_1 : \rho_4 \qquad 22$$

$$m_7^3 : m_8^3 :: \mu v_8^2 : \mu v_7^2 :: \rho_7 : \rho_8 \qquad 23$$

These proportions furnish the following indications respecting the order and rationale of cosmic development.

17_1. Earth, m_3, has a subsidence *vis viva*, which bears the same ratio to the mean subsidence *vis viva* of Sun (m_0), Earth and Jupiter (m_5), as æthereal *vis viva* in Earth's orbit (μv_3^2) bears to *vis viva* of luminous radiation (μv_λ^2). The early establishment and segregation of the terrestrial nucleus are also indicated by the position of the orbit in the centre of the belt of greatest condensation (secular perihelion of $m_1 =$ $\cdot 2974 \rho_3$; secular aphelion of $m_4 = 1\cdot 7365 \rho_3$), and by the equivalence between the radial momentum which luminous undulation tends to give the Earth ($m v_\lambda$) and the mean momentum which Earth's orbital velocity tends to give to Sun and Jupiter ($v_3 \sqrt{m_0 m_5}$). I called attention to the first of these facts, and to its importance, in 1877, (Proc. Amer. Phil. Soc., xvi, 501). Faye has introduced it into his late modifications of the hypotheses of Kant and Laplace (*Comptes Rendus,*

April 21, 1884). The additional confirmation which it lends to the Mosaic cosmogony is noteworthy. It may also be well to remark that the mean value of Sun, Earth and Jupiter, $(m_0 m_3 m_5)^{\frac{1}{3}}$, is about $\frac{1}{60}\, m_0$, thus including the whole planetary mass, together with about $\frac{1}{16}$ of Sun's mass.

18_1. The subsidence *vis viva* of Jupiter (m_5) is to the mean subsidence *vis viva* of Sun, Earth and Saturn (m_6), as æthereal momentum with orbital velocity at mean centre of gravity of Sun and Jupiter (μv_β), is to æthereal momentum with limiting orbital velocity at Sun's surface (μv_0). The significance of this fact is increased by Jupiter's central position in the primitive nebula, between Neptune in conjunction and Uranus in opposition.

19_1. Saturn's orbit embraces the primitive nebular centre of planetary inertia ($\sqrt{\Sigma \mu \rho^2 \div \Sigma \mu} = \rho_6$). The subsidence *vis viva* of Jupiter is to that of Saturn as æthereal *vis viva* at Sun's surface is to that at Kant's limit.

20_1. The predominating planetary influence of Jupiter, the tendency to grouping in pairs, the probability of segregation at the centre of the inter-asteroidal belt while Mercury and Mars were still zodiacal rings, mutual equilibrating activity and the influence of "subsidence" as taught by Herschel (Outlines of Astronomy, sec. 872), are shown by (20) and by the deduced equation $m_5 \times (m_1 + m_4) = (m_2 + m_3) \times (m_7 + m_8)$. It will be seen that this relationship is independent of any influence of the chief centre of nucleation (m_0) and the nebular centre of planetary inertia (m_6). The relations of subsidence *vis viva* among the groups introduce the ratio of oscillating æthereal volume (V_l) to corresponding subsidence-volume (V_μ).

21_1. The subdivisions of the foregoing groups show the influence of reciprocal harmonic *vis viva*. At the primitive centre of condensation, the subsidence *vis viva* of Venus is to that of Earth, as the product of Venus's mass by æthereal *vis viva* in Earth's orbit, is to the product of Earth's mass by æthereal *vis viva* in the orbit of Venus. We may also state 21 under the following form:

$$m_2^2 : m_3^2 :: \mu v_3^2 : \mu v_2^2.$$

This shows that the products of gravitating potential by ætherial orbital *vis viva*, for the two central planets of the dense belt, are equal.

22_1. In the segregation of the outer zodiacal rings of the dense belt, the reciprocity is that of simple æthereal orbital *vis viva*, the subsi-

dence *vis viva* of Mercury bearing the same ratio to that of Mars, as æthereal *vis viva* in the orbit of Mars bears to æthereal *vis viva* in Mercury's orbit.

23_1. In the segregation of the two outer rings of our stellar system, the same reciprocity is shown, the subsidence *vis viva* of Uranus bearing the same ratio to that of Neptune, as æthereal *vis viva* in Neptune's orbit bears to æthereal *vis viva* in the orbit of Uranus. Solving the foregoing proportions (17–23), we get harmonic values for the several planetary masses which agree very closely with the latest astronomical estimates, as is shown by the following table:

	Harmonic.	Astronomical.	Computers.	
$m_0 \div m_1$	4507467	4480040	Encke.	24
$m_0 \div m_2$	385313	385395	Newcomb-Leverrier.	25
$m_0 \div m_3$	327704	326800	Newcomb.	26
$m_0 \div m_4$	2854790	2848277	Hall.	27
$m_0 \div m_5$	1056·27	1050	Leverrier.	28
$m_0 \div m_6$	3499·01	3482	Hall.	29
$m_0 \div m_7$	22530	22600 ± 100	Newcomb.	30
$m_0 \div m_8$	19403	19380 ± 70	Newcomb.	31

In the ordinary astronomical tables of planetary elements, the planetary masses in the dense belt are deduced from their ratio to Earth's mass, on the old hypothesis that $m_0 \div m_3 = 354936$, while the extra-asteroidal masses are deduced from their ratio to Jupiter's mass. To remedy this inconsistency and at the same time to give values which are accordant with modern investigations, I have made such changes in the "Astronomical" column as are required by Newcomb's estimate of Earth's mass, which represents the greatest amount of labor, practical experience and mathematical ability that have ever been directed toward the solution of the problem. It is, moreover, within the limits of uncertainty which are still left by the discussions, so far as yet published, of the last transits of Venus.

The greatest difficulties in the astronomical estimation of mass, are presented by Mercury and Venus, on account of the absence of any known satellites. The two latest estimates for Venus are those of Leverrier ($m_0 \div m_2 = 379479$) and Newcomb (391310). The close agreement of the harmonic estimate with the mean of these two estimates, is, to say the least, very curious and interesting. The precise accordance in the cases of Uranus and Neptune, the closeness of agree-

ment in the cases of Earth and Mars, and the fact that each of the harmonic estimates is within the limits of probable error, are also satisfactory, especially when we consider that no allowance is made in the theoretical column for possible modifications by secondary harmonies, dependent upon planetary perturbations and orbital eccentricity, which are important elements in astronomical computation.

The simplicity of the principles which have led to the discovery of these relations may well encourage further investigations in the same field. The application of some of the higher branches of mathematical analysis and especially of spherical harmonics may, perhaps, lead to results which will not only be interesting, but will also be doubly valuable, inasmuch as theoretical anticipations can be readily tested by practical observation.

Bode's law, notwithstanding its failure in the case of Neptune, may, perhaps, be a partial expression of a more extensive and more general law. Indeed within the solar system, planetary or belt positions are nearly represented by the series 4, 7, 10, 16, 28, 52, 100, 196, 292, in which there are two equal intervals at the outer limit as well as two at the inner limit and the intermediate positions follow Bode's progression.

One of the simplest forms of cumulative harmonic influence is based upon simple harmonic motion, in which synchronous oscillations introduce the ratio $\pi \div 2$. If we take Earth and Neptune at their respective present points of incipient subsidence (secular aphelion) we find that the first and second harmonic submultiples of Earth's position are in the secular orbits of Venus and Mercury, the first multiple is in the orbit of Mars, and the fifth multiple is in the orbit of Saturn; the first submultiple of Neptune's position is in the orbit of Uranus, and the fourth in the orbit of Jupiter; the geometric mean between the second and third submultiples of Neptune is in the orbit of Saturn, while the geometric mean between the third and fourth multiples of Earth is in the orbit of Jupiter.

If we take the cumulative harmonic influence, which begins with Sun's semidiameter, with $\frac{\pi}{2}$ of the Kantian diameter, or the Kantian semicircumference, as the second term, the first five terms of the progression are ρ_0, $\pi L \rho_0$, $\pi^2 L^2 \rho_0$, $\pi^3 L^3 \rho_0$, $\pi^4 L^4 \rho_0$. The second term is in the belt of greatest condensation; the third bears the same ratio to the solar modulus of light as ρ_0 to ρ_k; the fourth is π times the solar modulus of light; the fifth is in a region of predominating stellar influence.

If we determine, by the method of least squares, the law of planetary progression, we find that it is suggestive of harmonic paraboloidal subsidence. Suppose Sun to be in the focus of a paraboloid of revolution, with a directrix plane at $\frac{\rho_0}{3}$ and a vertex at $\frac{\rho_0}{6}$. Suppose the nearest fixed star (presumably α Centauri) to be in the axis of the same paraboloid. Take 39 numerical abscissas of the form $A_n \xi \eta^n \zeta^{n2}$, with $A_0 = \xi = \frac{1}{6}$, $A_{19} = \frac{4}{9} L\rho_0$, $A_{38} = LM = \pi^2 L^4 \rho_0$. One third of the abscissas ($A_0 \cdots A_{12}$) are within the solar photosphere; one third ($A_{13} \cdots A_{25}$) are extra-solar and inter-asteroidal; one third $A_{26} \cdots A_{38}$ are extra-asteroidal and inter-stellar. The next abscissa (A_{39}) is in a region of predominating stellar influence, approximately, and *perhaps* exactly, in the locus of α Centauri.

The twenty-seven extra-solar and inter-stellar abscissas may also be divided into three equal suggestive groups, $A_{12} \cdots A_{20}$ being inter-planetary; $A_{21} \cdots A_{29}$ having significant planetary relations; $A_{30} \cdots A_{38}$ being extra-planetary. The middle group ($A_{21} \cdots A_{29}$) represents, respectively, $\frac{1}{2}$ Mercury, $\frac{1}{2}$ Venus, $\frac{2}{3}$ Earth, $\frac{3}{4}$ Mars, $\frac{4}{5}$ Asteroid, $\frac{5}{6}$ Jupiter, $\frac{6}{7}$ Saturn, $\frac{7}{8}$ Uranus, $\frac{7}{6}$ Neptune, the indicated loci being all within orbital limits. We find here, as in the Bodeian series, two equal numerators at the outer limit, where the harmonic mean of the two coefficients is unity, as well as two at the inner limit, where the harmonic mean is $\frac{1}{2}$. The coefficients $\frac{1}{2}$, $\frac{2}{3}$, etc., represent successive and progressive harmonic rupturing tendencies, inasmuch as particles falling toward a cosmic focus from a distance nr, would acquire the dissociative velocity, $\sqrt{2gr}$, at $\frac{n}{n+1}r$. The reciprocal character of the Saturnian and Neptunian coefficients furnishes an indication of such retrograde tendencies as we may naturally look for at the outer limits of a planetary system.

Although it is impossible, at present, to anticipate with certainty the precise way in which undiscovered harmonic influences will be manifested, it may be possible to show the probable existence of such influences and where to look for them. The tendency to make absolute any close approximation to simple numerical relations, which is found in Jupiter's satellites, should likewise often prevail in planetary motions. The number of such tendencies among the cosmical masses and positions is so great that it is difficult, for want of definite criteria, to judge of their relative importance. It may, perhaps, finally be found that they are all satisfied by adjustments of orbital eccentricity.

BACKWATER.

By C. H. PEABODY,
Assistant Professor of Applied Mechanics, Massachusetts Institute of Technology.

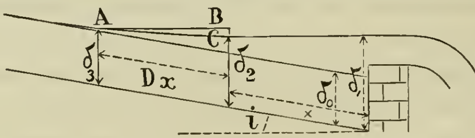
In Rankine's Steam Engine there is given the following equation for determining the curve of the backwater produced by a dam thrown across a channel of uniform width and declivity:

$$x = \frac{\delta_1 - \delta_2}{i} + \left(\frac{1}{i} - \frac{2}{f}\right)(\psi_1 - \psi_2)\,\delta_0\,;$$

in which x is the distance from the dam at which the water has the depth δ_2, δ_1 is the depth immediately above the dam, δ_0 is the original depth of the stream before the dam was introduced, i is the inclination of the bed and f is the coefficient of friction; φ is the following function of $r = \frac{\delta}{\delta_0}$:

$$\varphi = \int \frac{dr}{r^3 - 1} = -\tfrac{1}{6}\,\text{hyp. log.}\left\{1 + \frac{3r}{(r-1)^2}\right\} - \frac{1}{\sqrt{3}}\tan.^{-1}\frac{2r+1}{\sqrt{3}}$$

The equation given may be derived in the following manner. In the figure let δ_2 represent the depth at the distance x from the dam,



and δ_3 the depth at the distance $x + \Delta x$. If A is the area of the cross-section of the stream and b the wetted perimeter, then the loss of head on account of friction is $\frac{fb\Delta x}{A} \cdot \frac{v^2}{2g}$, in which v is the mean velocity between A and B. Now there is a fall at the surface of the stream of $BC = h$, and since the velocity changes from v_3 at A to v_2 at B, there is an available head from that source of $\frac{v_3^2 - v_2^2}{2g}$, consequently

$$\frac{fb\Delta x}{A}\,\frac{v_2}{2g} = h + \frac{v_3^2 - v_2^2}{2g},$$

$$h = \frac{fb\Delta x}{A}\,\frac{v^2}{2g} - \frac{v_3^2 - v_2^2}{2g}.$$

Since the width remains constant the velocities are inversely as the depths, consequently

$$v_3^2 - v_2^2 = \frac{\partial_2^2 - \partial_3^2}{\partial_3^2} v_2^2 ,$$

and it is apparent from inspection of the figure that

$$h = \Delta x \sin. i - (\partial_2 - \partial_3).$$

Substituting these values in the equation above,

$$\Delta x \sin. i - (\partial_2 - \partial_3) = \frac{fb\Delta x}{A}\frac{v^2}{2g} - \frac{(\partial_2 - \partial_3)(\partial_2 + \partial_3)}{\partial_3^2}\frac{v_2^2}{2g},$$

$$\therefore\ \Delta x = \frac{1 - \dfrac{\partial_2 + \partial_3}{\partial_3^2}\dfrac{v_2^2}{2g}}{\sin. i - \dfrac{fb}{A}\dfrac{v^2}{2g}} (\partial_2 - \partial_3)$$

If A approaches B, Δx approaches dx and at the limit may be written dx; at the same time $\partial_2 - \partial_3$ becomes $d\partial = \partial_0\, d\left(\dfrac{\partial}{\partial_0}\right) = \partial_0\, dr$. For ∂_3 and ∂_2 may be written the variable ∂, and for v_2, v. As an approximation i may replace sin. i, and $\dfrac{1}{\partial}$ may be used for $\dfrac{b}{A}$ if the stream is considerable wider than deep. Making these changes

$$dx = \frac{1 - \dfrac{2}{\partial}\dfrac{v^2}{2g}}{i - \dfrac{f}{\partial}\dfrac{v^2}{2g}} \partial_0\, dr .$$

Since the velocities are inversely as the depths $v = \dfrac{v_0\,\partial_0}{\partial}$. But before the stream was dammed the slope just overcame the friction, or

$$\sin. i = \frac{fb}{A}\frac{v_0^2}{2g} \quad \therefore\ \frac{v_0^2}{2g} = \frac{\partial_0 i}{f}.$$

Substituting for v

$$dx = \frac{1 - \dfrac{2i}{f}\dfrac{\partial_0^3}{\partial^3}}{i - i\ \dfrac{\partial_0^3}{\partial^3}} \partial_0\, dr ,$$

$$dx = \frac{1}{i}\left(1 + \frac{1 - \dfrac{2i}{f}}{r^3 - 1}\right) \partial_0\, dr .$$

Integrating between the limits r_1 and r_2,

$$x = \frac{\delta_1 - \delta_2}{i} + \left(\frac{1}{i} - \frac{2}{f}\right) \left[\int_{r_2}^{r_1} \frac{dr}{r^3 - 1}\right] \delta_0,$$

the equation at the head of the article.

Rankine also gives the following approximate integration:

$$\varphi = \int \frac{dr}{r^3 - 1} = \int \frac{dr}{r^3} + \int \frac{dr}{r^6} + \int \frac{dr}{r^9} + \text{etc.}$$

$$\therefore \quad \varphi = -\frac{1}{2r^2} - \frac{1}{5r^5} - \frac{1}{8r^8} - \text{etc.}$$

and derives from it a table which much reduces the labor of calculation.

r	$-\varphi$	r	$-\varphi$
1·0	∞	1·8	·166
1·1	·680	1·9	·147
1·2	·480	2·0	·132
1·3	·376	2·2	·107
1·4	·304	2·4	·089
1·5	·255	2·6	·076
1·6	·218	2·8	·065
1·7	·189	3·0	·056

In the Steam Engine the values for φ are given with positive sign; also the results of the integration of $\int \frac{dr}{r^3 - 1}$, both exact and approximate, have the positive sign, which is evidently an error.

The value of $\frac{2}{f}$ is given as 264.

Attention is called to the fact that the first member of the right hand side of the equation deduced above, is the distance back of the weir at which the depth δ_2 would occur if the surface of the water were horizontal. If the slope is 1 in 264, that is if $\frac{2}{f} = \frac{1}{i}$, the second term vanishes. For a greater declivity it becomes negative, showing a rise of water toward the weir.

THE PANAMA INTEROCEANIC CANAL.

By Charles Colné,
Secretary of the American Committee of the Universal Interoceanic Panama Canal Company.

[Read at a Special Meeting of the Franklin Institute, Wednesday, October 22, 1884.]

Mr. Hector Orr in the chair.

Mr. Colné:—The question of shortening communication between Europe, the United States and the West Coast of Central and South America by means of an artificial waterway has attracted, for years, the attention of many minds. Its vast importance to commerce and navigation was long ago felt; in this age of rapid transit it becomes a necessity. The advantages to be derived from such a means of communication can scarcely be computed, for past experience in similar routes has demonstrated that the most sanguine expectations have been met and even surpassed.

Many have been the projects, explorations upon explorations have been made, some of a very crude nature, but of late years valuable data have accumulated upon nearly all practicable passages. This question, like many important ones is not new. It has taken many years, however, to bring it to a practical solution. It has required many researches, it has cost much labor and in several instances sacrifices of life.

Many of us will remember the recent agitation by the press and by discussions in our public halls of the best means to unite the waters of the Pacific Ocean with those of the Atlantic. This, to the public at large, was rather a new question. History tells us, however, that over three and half centuries ago the proposition of cutting a waterway across the American Isthmus had already assumed importance.

Vasco Nuñez de Balboa, in 1513, having taken possession of the Pacific Ocean, bethought himself of effecting a passage through the rivers of Darien, but his death, a short time afterwards, put a stop to this project.

In 1523, ten years afterwards, Fernando Cortez, then master of Mexico, proposed a waterway through the Isthmus of Tehuantepec. He employed Gonzalo Sandoval to make a very careful survey of the land. Even when the Emperor Charles V withdrew the Government of Mexico from his hands, he nevertheless continued to urge his project for a maritime canal at the place he had selected. But as the Emperor at that time was more engrossed with the, to him, more

important question of getting revenue from New Spain, the idea of spending money for the benefit of that country was altogether unwelcome. To this proposition he remained as indifferent as he had been before to that made to him by Angel Saavedra, in 1520, to adopt the idea of Balboa to cut the Darien Isthmus. It is a remarkable fact that these two first proposed routes for interoceanic canals should represent the two extreme points where crossings could be effected.

In a document submitted to the Portuguese Congress under the title "The Cutting of the Isthmus of Panama in the Sixteenth Century," by Pereira de Païva, it is stated that since 1550 four different routes had been proposed by a celebrated Portuguese navigator, Antonio Galvao, who had published a book entitled "A treatise upon the sundry and circuitous ways by which pepper and spices have been sent, and upon ancient and modern discoveries made up to the year 1550." In this Galvao states that a maritime canal can be cut in four different places. First, between the Gulf of Uraba and the Gulf of San Miguel; second, through the Isthmus of Panama; third, along the San Juan, through Lake Nicaragua; and fourth, through the Mexican Isthmus. It would then seem that the principal routes were already known before the end of the sixteenth century. The surveys, however, up to this time were far from being thorough, they simply gave general ideas of the routes to follow. Many were the difficulties in the way of obtaining positive information, the country was totally unknown. Several explorers started out to get the desired information, but they all met with partial success, only. Among them were Morales, Meneses, Espinosa, Pedrarias and Andagoya. Nothing definite was ever brought back by any of them.

The seventeenth century was rather indifferent to the question of interoceanic communication. Towards the end of the eighteenth century the idea was again revived. England, then, as now, alive to any question of self-interest, thought it would be of great value to her if she could succeed in controlling a passage from ocean to ocean. At this period a passage through a canal was not so much thought of as the possibility of making rivers available for this purpose. Nicaragua was looked upon as offering these advantages. So in 1778 by methods not unlike those in use by her at the present day, she sent Nelson on an expedition against Nicaragua, in order to take possession of the territory. This expedition, however, was not numbered among the

victories of the British; it proved to be very disastrous, and Nelson nearly lost his life.

It may be said that in 1780 the first technical explorations started under the orders of Charles III, King of Spain, with the French engineer, Martin de la Bastide as leader, and the Spanish engineer, Manoel Galistro, for the purpose of having a canal cut through the Isthmus of Panama. On their return to Spain they found the country plunged into political questions arising out of the French revolution, the death of Charles III taking place soon afterwards, nothing came of this effort. The Viceroy of New Spain, Antonio de Bucareli, in the meantime had shown a favorable disposition by having several surveys made by engineers Corral and Cramer.

At the beginning of the present century, the celebrated Humboldt, after having been personally upon the spot, advocated, in 1804, with Admiral Fitz Roy, the Darien route. These well known names carried many partizans with them, and thus, for the first three quarters of this century, the Darien Isthmus has been the stumbling-block in the way of accomplishing interoceanic communication. It took the two very laborious surveys of Messrs. Wyse and Reclus in 1877 to remove the Darien route out of the way, and it was then discovered that the projects had been based upon erroneous suppositions.

In 1814 the Spanish Cortes passed a resolution ordering the Viceroy of New Spain to undertake the cutting of a canal on the Isthmus of Tehuantepec. Mexico having shortly afterward, become an independent State the burden of this work fell upon the new Government. In 1821 General Orbegoso surveyed the line, and in 1842, under Santa Anna, Don José de Garay, after having obtained a concession for a canal, continued the surveys. His son, Don Francisco de Garay came to Paris to advocate this line before the International Commission of 1879.

Napoleon Garella, a French mining engineer, first gave positive data upon the Panama Isthmus, for the purpose of building a railroad or a canal. This project found favor with the French army engineer corps, and received their endorsement.

The two route-surveys were completed in 1844. A French company paid the expenses of the survey and decided to build a railroad. Through delays and the untoward events of the French Revolution of 1848 the proposition fell through. The concession having expired, an American company obtained a new one, and built the Panama rail-

road of the present day. Thus do we owe to an American enterprise the possibility of a successful crossing of this heretofore almost impassable strip of land. To my friend, the late Col. G. M. Totten, we owe the successful accomplishment of this work. His undefatigable perseverance and tenacity conquered the many difficulties in his way. He lived in New York, up to last May, to a ripe old age, respected and loved by all. At the time of his death he held the position of consulting engineer of the American Committee of the Panama Canal Company in New York. Philadelphia also had among her honored citizens up to a short time ago, a worthy co-laborer of Col. Totten, Engineer J. C. Trautwine. To both of these men the California gold seekers of 1848 and '49 owe a debt of gratitude, for having lightened their tedious way across the Isthmus. Among the directors of the Panama Railroad may be found the name of Mr. Theod. J. de Sabla, son of Mr. de Sabla, who patronized and proposed the first railway communication across the Isthmus. He has been a resident of New York for many years.

In 1850 Gen. Barnard, of the Army Engineer Corps, after returning from Tehuantepec, where he had been surveying a line for a railroad, gave his opinion that this Isthmus was the most unfavorable spot for a crossing between the two oceans.

Honduras was then thought to have many advantages. The surveys of Squiers, Trautwine and Jeffer dispelled such an idea. Nicaragua was then taken up; the San Juan River, the great lake and the low altitude of the land were thought to be favorable conditions. Engineers Childs and Fay, after having made technical surveys, reported favorably. For thirty years this route remained in favor, and to the present day has some adherents still. A French engineer, Felix Belly, and an American engineer, Crossman, also explored the Nicaragua line, the latter lost his life trying to cross the bar at Greytown.

In this country the route advocated by Humboldt and Fitz Roy through the Isthmus of Darien had many adherents. Among them Mr. F. M. Kelly, a gentleman now living in New York, spent large sums of money in defraying the expenses of several surveying parties in that section. The Trautwine expedition in 1852; the Lane and Kennish expedition in 1853, under his auspices came back, however, without bringing a satisfactory knowledge of the Isthmus. Dr. Cullen, a strong advocate of this route, stated then that he thought a low valley existed through which the Caledonian Bay could be reached

by the way of the Savannah. This confidence induced many new explorers to take the field, but they were nearly all unfortunate.

Patterson and his Scotchmen died in the neighborhood of the Lara and the Savannah. The American engineer, Strain, started for the Caledonian Bay, but lost his way, his instruments and provisions, then mistook the Chucunaque for the Savannah. For days and days he and his companions, wandered through a merciless and impenetrable forest, seventeen of his men died; Strain himself, in consequence of the terrible hardships he had endured in that unfortunate expedition, died on landing in the United States.

In 1853, Gisborne, driven away by the natives of the country, did not bring back anything of value. Prevost's men were killed by the Indians. From all these expeditions nothing but indefinite and contradictory information was brought back.

In 1858, General Michler, of the Engineer Corps, took up the surveys of Trautwine and recommended a line running through the valleys of the Truando and Atrato rivers.

In 1870, our Government, finding none but conflicting reports from previous surveys, decided to have a thorough and new one made fcr every proposed route. With a praiseworthy liberality, ships, men and treasure were put at the disposal of the commanders of these expeditions. Commodore Shufeldt went to the Isthmus of Tehuantepec, Commanders Hatfield and Lull to Nicaragua, Lull continuing his researches to the Isthmuses of Panama, Darien and a small part of Cauca, were also explored by Commander Selfridge and Lieutenant Collins. It took three years to complete these explorations.

In 1871, the first Geographical Congress took place at Antwerp. General Heine, an American, advocated Mr. Gogorza's project, who with Lacharme were said to have found an exceptionally favorable route on the Darien Isthmus through the rivers Tuyra, Atrato and its affluent the Caquirri. At this meeting of the Congress a resolution was adopted recommending attention to Mr. Gogorza's route. At the next meeting his project again received favorable consideration.

In 1875, however, Mr. Ferd. de Lesseps expressed an opinion that a great mistake had been made in proposing none but canals with locks and strongly urged the adoption of a sea-level canal, like the Suez, doing away with locks. This he maintained was the only kind of canal capable of meeting the requirements of modern commerce removing all delays and obstacles inherent to locks. This new idea after a

thorough discussion caused the Congress to adopt a resolution recommending a canal "offering the greatest facilities of access and circulation." Since the proposition had such a novel character from the old settled ideas of canal, this Congress naturally conservative did not think it prudent at this time to go any further. This apparently harmless resolution, however, lead the Geographical Society to inquire into the matter of interoceanic canal with new ideas. It was soon discovered that the geography of the Isthmus was quite incomplete in several parts, though many explorations had already been made. For the lack of sufficient information no recommendation could be made, and to obtain such information much more money would be required than was at the disposal of the Society. A committee was appointed, with Mr. de Lesseps as chairman, to continue the researches. While they were at work Lieutenant Wyse and General Türr succeeded in forming a private company to furnish the money to continue the exploration and surveys. During the same year an expedition was fitted out under the command of Lieutenant Wyse. After many painful journeys and sufferings the party succeeded in going over the supposed favorable route of Gogorza, but no special advantages were discovered. Messrs. Wyse and Reclus, the latter also a lieutenant in the French navy, thought that a better route could be found further west between the Tuyra and the Acanti bay. The rainy season having set in operations had to be suspended. Three members of this expedition Messrs. Bixio, Brooks and Musso, died during this laborious work. Messrs. Wyse, Reclus, Gerster and Lacharme, only returning. The following year the work was again taken up. While Lieutenant Reclus finished up the surveys of the Tuyra-Acanti route, Lieutenant Wyse explored the San Blas region and the Panama Isthmus was again gone over. The void left by former explorers was now filled and complete information obtained.

Thus through private efforts the long wanted and important technical data were supplied, and also through private efforts did Mr. de Lesseps obtain the first capital for the prosecution of the work on the Panama Interoceanic Canal, an enterprise of vast magnitude and of world-wide importance. Surely a man able to command such confidence, repeated three times since, in the shape of new and large subscriptions, cannot be the deceiver which a part of the press of this country has represented him to be. A nation which shows such confidence in the man, as France has, must certainly have good reasons for it.

France has not only furnished the larger part of the capital, but she also has furnished the brain for carrying out a work which has attracted the attention of the whole world for so long a time. Instead of deriding her let America praise and encourage her, for we are the nation that will derive the greatest advantages from such a work.

If an enterprise has merit to commend it to capitalists, what need is there to appeal to government aid as has lately been done here by persons seeking to open another interoceanic canal? It is not compatible with the spirit of our government to be paternal, the people appoint their representatives, not rulers. If the people of this country want a second canal and they think there will be sufficient business for it, they will not be slow in manifesting their sentiments on that question to their representatives. In the meantime if foreign capital is willing to open a canal let us not make a laughing stock of ourselves by making a causeless war upon, I may say, such a humanitarian enterprise, Americans in general are too fond of fair play to countenance anything having the flavor of unfounded prejudice.

On the eve of seeing a great waterway opened to the commerce of the world, it may not be uninteresting to look back and see what makeshifts trade has had to resort to for transporting goods in former times.

When Pizarro had conquered the empire of the Incas, and Peru had become one of the richest jewels of the Spanish crown, trade with the mother country naturally increased wonderfully.

How to avoid the long and dangerous navigation through the Straits of Magellan, or around Cape Horn, became a question of much importance. It was quite natural that the narrow strip of land discovered by Balboa should be looked to for a solution of these obstacles and as offering a rapid and sure passage. And since this route could be shut out from use by any but the Spaniards, to the exclusion of all heretics, it had special merits in the eyes of this fanatical people. The plan adopted was this. It was agreed that every year a fleet of galleons should start at a given time from Spain for the east coast of the Isthmus, and at the same time another fleet should leave Peru for the western coast. Between the two landing places a road was to be opened overland to transport from ship to ship the valuable cargoes and passengers of these fleets. What point should be selected over this Isthmus extending over 1,500 miles for the opening of a road? Even at this early date the Isthmus had been thoroughly explored, and had it not been that the records of the surveys of the wonderful

Adelantados (Governors) had remained in the secret archives of the Spanish government nothing would have been necessary to rediscover.

The route selected ran over the ground which has since been recognized, by thorough surveys, from Darien to Tehuantepec, to be the most suitable. Nothing could induce the "Conquistadores" to adopt any other line than that which was subsequently selected for the Panama railroad. The Panama canal, the colaborer of the railroad, will also follow substantially the old route of the Spaniards. The roads adopted for this primitive Isthmian traffic started from nearly the same points on the eastern coast and ended at Panama.

When Morgan, the fillibuster, in 1670 invaded the country, the road was made up of two pathways, one starting on the Atlantic from Porto Bello the other from Chagres, both ending in Panama, thus forming an open angle with Panama at the Summit.

From Porto Bello a mule-path wound its way through the forest, narrow, abrupt following hills and valleys, paved, however, in parts where the soil was too marshy, passing through and stopping at a few settlements, Pequeni being the principal one, where mules were changed. By this route were sent, on muleback all the dispatches, the gold and silver bars, from Peru, light merchandise and the mercury, then monopolized by the Viceroy of Peru, which was such an essential material for the rich mines of that country. This route was the rapid transit of the day.

From Chagres to Cruces the route was by way of the river. Large flat boats, sixty feet long and twenty-five feet wide, ran up and down these waters. Upon these boats all the heavy and bulky merchandise was loaded, and also the unfortunate negroes the Genoese merchants were sending to the mines of Potosi "by and under the authority of court." When Cruces was reached, the river not being navigable beyond this point, goods were unloaded and stored in warehouses. Although but a distance of twenty-two miles remained to reach Panama, all merchandise had to be transported there through narrow paths on muleback. To supply the wants of the traffic of these two roads no less than two thousand mules were required. These two roads were so impenetrable and so little known that at the time the freebooter Morgan made his dastardly descent upon Panama, among his twenty-two hundred French and English followers none were found capable to guide the expedition, although many of them

were well acquainted with the coast. He had to go to the Island of Saint Catherine where he found two convicts whom he set free and they consented to guide him. At no time during the Spanish rule does it ever appear that any better means of communication existed than a mule-path. It took Morgan nine days to go across and reach Panama. After the separation of the Spanish colonies from the mother country the necessity of intercourse between them having ceased, the Isthmus route was forgotten more than ever, and traffic between Europe and the Pacific coast took the Cape Horn route again. This old Isthmian road might have remained forgotten to this day had it not been that the discovery of gold in 1848, in California, gave a new and wonderful impetus to travel across the Isthmus. Porto Bello and Chagres, silent for so many years again became alive with surging and eager crowds of gold seekers, landing from constantly arriving ships. Bold adventurers pushed through the forest, but the vigorous growth of the climate had obliterated all traces of the former pathways with the exception of the few paved places in the marshes. With the eagerness the gold seeker displays in all countries these indomitable men finally cut their way through to Panama. This road having become an absolute necessity to our Pacific coast, it was not long before our people with their well-known pioneer spirit and enterprise completed the railroad which was then in process of construction across the Isthmus. The Panama Railroad now carries merchandise and passengers through in a few hours, not so rapidly as some of our roads in the United States perhaps, but compared to the old time modes of travel this railway is a vast improvement. Since the canal work has been going on the capacity of this road has been wonderfully increased.

We have had the mule-path with its discomforts, slowness and impediments, then the railway for the transportation of light merchandise and passengers.

There remains a desideradum for the cheap transportation of heavy and bulky merchandise in a rapid and uninterrupted manner. The Panama canal will fill these conditions.

HISTORY OF THE PANAMA CANAL.

In 1879 Count Ferdinand de Lesseps made an appeal to the several nations to send delegates to a proposed Congress to meet in Paris and

deliberate upon the different routes proposed for interoceanic communication between the Atlantic and Pacific Oceans.

On the 15th of May, of that year, pursuant to his call, this Congress met in Paris for organization. The following countries sent representatives: Germany, 1; England, 6; Austria-Hungary, 2; Belgium, 6; China, 1; Costa Rica, 1; Spain, 5; United States, 11; United States of Columbia, 4; Guatamala, 1; Hawai, 1; Holland, 6; Italy, 3; Mexico, 1; Nicaragua, 1; Portugal, 2; Norway, 1; Russia, 2; San Salvador, 2; Sweden, 1, and Switzerland, 4. France was also represented, as well as the Colonies of Algiers and Martinique—Peru, owing to the war then raging did not send any delegates. Among the French representatives, 74 in number, 4 were from Scientific Institutions, 8 from Chambers of Commerce, 25 from Geographical Societies, 4 from the Diplomatic Corps, 23 Engineers, 6 from the Navy or Navigation Companies, and 4 Mining Engineers.

Among the foreign delegates, 61 in number, 21 were Engineers, 1 Mining Engineer, 7 from Chambers of Commerce, 12 from Geographical Societies, 11 Foreign Diplomatic Representatives, 1 Army Officer, 2 from Scientific Societies, and 6 from the Navy.

Mr. Ferdinand de Lesseps was elected President; Rear Admiral Daniel Ammen, from the United States; Sir John Stokes, from England, Vice-Admiral Likhatchof, from Russia; Commander Negri Cristoforo, from Italy, and Colonel Coello, from Spain, as Vice-Presidents; Mr. Henri Bionne, from France, General Secretary.

To facilitate the work of the Congress and to dispatch business "à l'americaine," as Mr. de Lesseps expressed it, five commissions were appointed. The first, on "Statistics," or probable business of the canal, and the participation of each nation. The second, on "Economical and Commercial Questions," the advantages each nation would derive from the canal, etc. The third, on "Navigation," the kind of ships that would be likely to go through the canal, the influence of the canal on future ships, the prevailing winds and currents, the climate, meteorology, and their effect on materials, etc. The fourth, and most important, on "Technological Questions:" Expenses for Construction, for operating, for repairs, facility and security of navigation.

This commission was divided into two sub-commissions, one to discuss and decide upon the route to be adopted, the other to make estimates of cost. The work of these two commissions was, in fact, to guide the deliberations of the Congress in regard to the route and the

style of canal to be adopted. The fifth commission was to determine upon the "ways and means" for operating the canal and the probable revenue. The fourth commission virtually decided the question of the Interoceanic Canal. It was composed of 42 members: 29 Engineers, 3 Mining Engineers, 2 Contractors, 2 members of Scientific Institutions, 2 members of Geographical Societies, 2 Officers of the Navy, 1 Officer of the Army, and one Foreign Minister.

The United States had a representation of 11 members: 2 Engineers, 2 Officers of the Navy, 2 members of Geographical Societies, 4 members of Chambers of Commerce, 1 member of a Scientific Society. Three of these representatives were members of the Technological Commission.

Each of the commissions, after full and free debates, had reports prepared by well-qualified persons, upon the different questions referred to them. These reports are replete with information, and are founded upon data furnished by competent persons from all nations. Each of these reports was submitted to the general meeting, and formed a basis for deliberations. The persons advocating the different canal schemes were afforded opportunities to appear before these commissions and explain their proposed routes.

The meetings of the Congress lasted from the 15th to the 29th of May, 1879. On the last day the following resolution was adopted:

> "Congress believes that the cutting of an inter-oceanic canal, with a constant level, so desirable for the interest of commerce and navigation, is possible; and that this maritime canal, to meet the indispensable facilities of access and utility which a passage of this kind should offer before all, shall be by the way of Limon Bay to Panama."

This resolution was adopted by the following vote, out of 98 members present:

Abstentions	12
Nays	8
Yeas	78
	98

Many of the absentees were persons who took no part whatever in the discussions and deliberations of the Congress.

It may, therefore, be said with safety, that with such an array of eminent men, experienced engineers, navigators, merchants, contractors and geographers, this decision has not been reached without mature

deliberation and a sufficient knowledge of the advantages and disadvantages of the different routes proposed.

Many projects were submitted, such as the route by Tehuantepec, by Mr. de Garay; the Nicaragua route, by Commander Lull and Mr. Menocal, with a modified project by Blanchet; the San Blas route of Mr. Kelley; the route by the Atrato and the Napipi, by Commander Selfridge. Mr. de Puydt also sent a communication regarding a route through Darien, starting from Puerto Escondido, to the Tuyra river, but for want of reliable data this route was put aside. The Panama route was presented by Lieutenants Wyse and Reclus.

After full and long discussions, but two routes were found to have sufficient merits for discussion—the Nicaragua and the Panama routes. It having been decided, however, that a canal with locks did not offer the advantages of a quick and unimpeded passage, it was finally cast aside and the sea-level route, without locks, tunnels, or any other impediments to navigation—the Panama route—was adopted. The only delay in transit—if such it can be called—that will be experienced in going through the Panama Canal will be the tide-lock at its mouth on the Panama side.

In the face of the decision of this Congress, composed as it was of men selected with the proper knowledge of the questions to be decided, our country being represented by well-qualified and well-known gentlemen, it does not seem reasonable that the voice of a few dissenters, whose interest lies in another direction, should have the precedence over such a majority of men of national reputation. The canal route has been selected, Mr. de Lesseps has organized his company, he has obtained the necessary capital for going on with the works, and, as Capt. Eads terms it, in his testimony before Congress, at Washington, "Mr. de Lesseps would belie his whole previous history if he did not push that work with all the energy he is capable of." A man who has built the Suez Canal in the face of the difficulties he had to encounter repeatedly, who with his well-known persistence has conquered them all, and has succeeded in giving to the world one of the greatest achievements of the age, can certainly be trusted to build the Panama Canal. Capitalists and the people have shown their faith in him by aiding him liberally with their money. Those who had faith in Mr. de Lesseps while he was struggling with the Suez enterprise have not lost faith in him yet; they have again come forward with their money to help him to build a new and more important canal. We, in the

United States, are to derive the greatest advantages of all the nations from the opening of such a canal; if we do not give it financial aid, we should, at least, from dictates of self-interest, if not from higher motives, give it our encouragement and wish it godspeed. For every year that the opening of a canal is delayed, Captain Merry estimates that our California coast loses $13,000,000, and he thinks that a tonnage of more than 3,000,000 tons can be relied upon for transit. The canal is an admitted necessity.

DESCRIPTION OF THE CANAL.

The canal commences at Colon (Aspinwall), running up to Gatun and to Dos Hermanas in a very long curve, almost a straight line, starting at the sea level in low lands, reaching Dos Hermanas with an elevation of land to 20 feet, in a gradual ascent, at a distance of $9\frac{2}{3}$ miles from Colon. From Dos Hermanas to Frijole, a distance of $17\frac{1}{3}$ miles from its mouth, the canal reaches the latter point at an average elevation of 40 feet, with the exception of a hill between Buhio Soldado and Buena Vista, reaching a height of 165 feet. From Frijole to Mamei, a distance of 24 miles from the mouth the line makes a bend, and reaches Mamei, with an average elevation of 50 feet, with intervening hills reaching to heights of 85, 100 and 118 feet. From Mamei to Matachin, 27 miles from Colon, the canal makes another easy bend, the height of the land averaging 55 feet, excepting a hill near Matachin of 168 feet. The balance of the line to Panama is comparatively straight. From Matachin to Culebra, a distance of 34 miles, the land becomes more undulating, with a series of hills reaching altitudes from 100 to 240 feet, and at the Culebra, reaching the highest point on the line, 330 feet.

From this altitude at Culebra the descent reaches to 30 feet at Rio Grande, a distance of 37 miles from Colon. From Rio Grande to La Boca the line again runs through low lands from 30 feet to the level of the ocean, having reached the distance of 42 miles from Colon. To reach the proper depth of water, dredging will be continued to a point near the Islands of Perico, being a distance of 46 miles from Colon.

The two ports, Colon and Panama, are to be improved so as to make the entrances easy of access. At Colon the port has already been improved very much; the channel has been dredged to the entrance of the Folks river, so as to allow the large American dredges to do their work in cutting the preliminary cut up to Gatun, where the Chagres

is reached. Quite a city has been built by the company between the Panama Railroad and the bay; it is known as Christopher Columbus. A large number of houses has been put up for quarters for the officers and employés of the Company. The principal street, Charles de Lesseps street, has a row of fine cottage houses for officers of the Company to live in. These houses were framed in the United States, taken down and again put up on the grounds of the new city. An extensive plot of ground has been filled in and reclaimed from the bay.

At the extremity of this land a wharf has been built, where the machinery, materials and food received for the Company will be landed. The Panama Railroad has a track extended to this wharf, so that all freight received can be sent at once to any point on the canal where works are going on.

Beyond the pier the filling has been continued so as to form a mole to protect ships while in the harbor from the effect of northern winds, which at times are quite violent and might cause damage and destruction to the shipping.

Dredging is now going on all over the bay, so as to give a sufficient depth of water at the wharf and at the entrance of the canal. Dredging is done with dredges belonging to the Canal Company. An American company has taken a contract for 39 millions cubic yards on this end up to Matachin, 27 miles from the mouth of the canal—the American Contracting and Dredging Company of New York. They have now three powerful dredges on the Isthmus built in the United States, and are building a fourth one in New York. The first cut made by these dredges has reached about three miles inland. A large number of workshops, storehouses, quarters, etc., have been erected in Colon, where machinery received from the United States and Europe is constantly being put up. The Company has also sub-offices in Colon. This place has become quite important, and the population has vastly increased The Canal Company has rebuilt a wharf for their own use. At Matachin other workshops have been erected for putting up and repairing machinery. At Gamboa a station has been established with a view of the construction of the large dam at this point. At Culebra, the highest point on the line, work has been commenced; the dumping of earth will be much facilitated from the fact of there being lateral valleys at a lower level than the canal. At Corosal another large American Company, the Franco-American Trad-

ing Company, has just closed a new contract for dredging 10 millions of cubic yards. Ten dredges are to be employed in this work. The balance of the sections is all under way. At Panama a channel, 325 feet wide, is to be dug to low water near the Island of Perico, so that ships will be able to enter into the canal at any time. At the mouth (La Boca) of the canal a tide lock is to be built, so as to maintain at all times a sufficient depth of water for ships to go in and out of the canal. This has been found necessary, owing to the high rise and fall of the tides.

The Chagres river, being subject to heavy floods during the rainy season, it has been decided to build at Gamboa a large dam, to retain the waters and let them out gradually by lateral outlets or canals. In summer the Chagres river has a flow of 460 cubic feet, which reaches to 21,200 feet in winter, and in some instances of heavy flood it has attained 56,500 cubic feet per second.

To let such a body of water into the canal would be inevitable destruction. Lateral canals will consequently be cut to carry off these waters, varying in size from 27 feet at the bottom up to 131 feet near the sea. These will run off the freshet waters without lowering or raising their level. The dam is to be 492 feet at the base; between two hills, the cerro-Santa Cruz and the cerro-Obispo, it is to be 150 feet high. The waters retained in a valley above this dam will reach to 35 billion cubic feet. The lateral canal will have sufficient capacity to carry off, by a tunnel cut through solid rock on its right bank, 14,000 cubic feet per second. At a slight depth below the bed of the river a solid rock bottom has been discovered. It is proposed to erect the dam upon this rock foundation. The silt and gravel overlaying it will be removed and washed away by narrowing the river, and thereby increasing the current so as to give it sufficient force to remove these loose materials.

The dam will require about 9 millions of cubic yards of earth and rock to build it. There will be no difficulty, however, to procure such a large amount of excavation, for the neighboring hills to be removed will produce from 52 to 65 millions of cubic yards. The slopes down stream are to be very slight and protected with large stones. This precaution is taken so as to avoid the earth being carried away by freshets while the dam is in course of construction. It is not expected to make this dam water-tight at first; but, by throwing earth on the upper

side, it will gradually fill up the interstices between the stones and secure this object.

Such dams have often been constructed by our California engineers engaged in hydraulic gold mining enterprises, and have stood the test of years. The cost of this dam is estimated at $1,600,000, with all the accessory work; an overfall for the surplus waters will be provided in the construction. It is expected that the water above the dam will rise in winter to 95 feet above the level of the canal, or about 115 feet during the usual freshets of November. There are, however, exceptionally heavy freshets (seldom, however), when water may then rise to nearly 200 feet above the level of the water in the canal. In such a case, the lateral outlet will pour out 14,000 cubic feet per second, reducing the volume to three quarters of what it would be without the dam.

When the canal crosses rivers, as it does at a number of places, lateral canals or artificial channels will be excavated, pouring together again the waters of the rivers.

In the lowlands of the Rio Chagres and Rio Grande the canal is to be 72 feet at bottom, 164 feet at water level. In the section between San Pablo and Pedro Miguel 78 feet at bottom and 102 feet at water level; at the great Culebra cut, 72 feet at bottom and 100 feet at water level. The slopes are not yet definitely settled upon, but are approximately known.

Recent soundings have developed the fact that much less rock exists than had been anticipated. This is especially the case at the cut of the Culebra, where soundings have shown the materials to excavate to be composed of semi-soft rock of schistose formation, lying in horizontal layers, in dry earth. This favorable nature of the soil will produce a vast economy in the price of excavations.

The excavations necessary to complete the canal will be, for cutting the canal and the ports proper, 143 millons cubic yards, and for the lateral or derivation cuts, 13 millions cubic yards; together, a cubic measurement of 156 millions cubic yards. Of this amount, 52 millions will be excavated with dredges, the cheapest mode of working, and 104 millions cubic yards are dry excavation of earth and rock.

To faciltate traffic on the canal, since it is not possible for ships to cross each other in transit, it has been decided to dig a large basin or siding, 3 miles long, about midway of the canal, near Tabernilla.

The Canal Company has purchased a controlling number of shares

LOCATION OF THE PANAMA CANAL

and Situation of the Works in course of construction

on June the 1st 1884

PROFILE ALONG THE AXIS OF THE CANAL

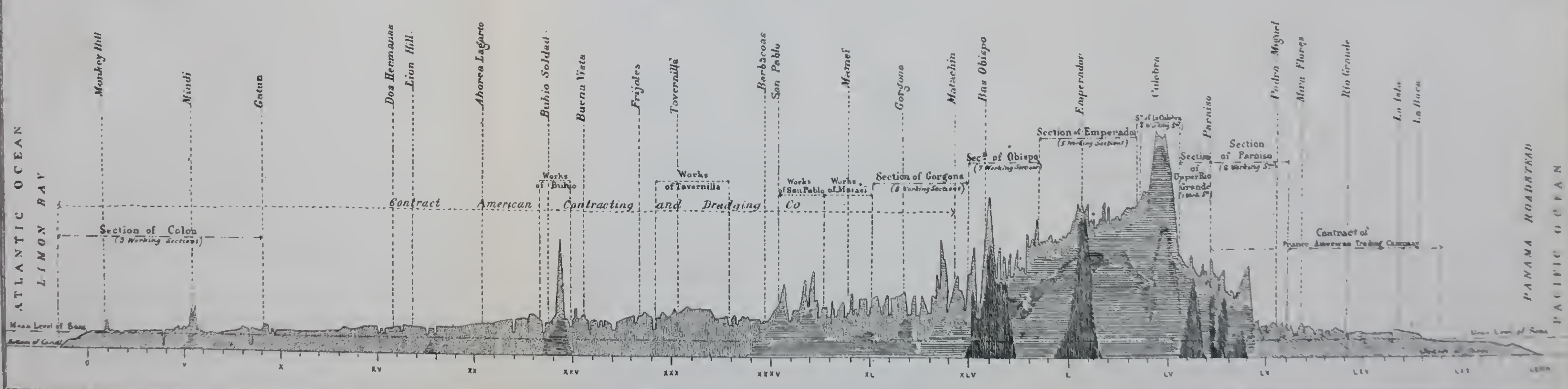
Heights amplified 100 times over lengths

Scales {Heights / Lengths

Loose earth

Solid earth.

Hard rock



of the Panama Railroad, and by this means has secured the help of this road for the transportation of machinery, materials, etc. This enables the works to be prosecuted at many points at once, since the railroad is almost parallel to the line of the canal at all points except between Gatun and Buhio Soldado, and on the Panama end where the canal runs on to La Boca, instead of ending in Panama, as the railroad does. In these places tracks have been laid to the main road.

At a later period it is proposed to excavate at each end of the canal, in the lowlands, two large basins where ships may lay by for repairs or for any other cause, thus producing safe and quiet harbors in smooth water. Each basin will be about 120 acres capacity. The total of excavations reported to September 1st, 1884, is 10,224,882 cubic yards. The Canal Company has put up a number of houses all over the line, for the use of their employés and workmen.

PROBABLE OPENING OF THE CANAL.

Mr. de Lesseps, the President of the Company, at the last annual meeting of the stockholders has again reiterated that, except in case of unforseen circumstances, the canal will be open for traffic in 1888. This is no idle promise. It is founded upon the assurance of the Chief Engineer in charge of the work at Panama, Mr. Dingler, a member of the Engineer Corps of France, endorsed by the opinion of the Technological Consulting Commission in Paris. The company does not depend upon manual labor, as would appear from the 20,000 men who are now employed on the line, in order to finish the canal by the time stated, but a large number of labor-saving machines have been ordered, such as dredges, excavators, locomotives, derricks, tug-boats, cars, dump-scows, steam dischargers, portable engines, etc.

The following averages of efficiency have been made up from experience and careful calculations, counting twenty-four working days to a month: The large American dredges of the American Dredging and Contracting Co., 78,000 cubic yards per month for each, or 3,250 yards per day. Medium size French dredges, 60 horse-power, 26,000 cubic yards each per month, or about 1,080 yards per day. Large French dredges, 180 horse-power, average 52,000 cubic yards each. The theoretical average of excavators of the different styles on the Isthmus is 1,300 to 1,500 cubic yards per day, but the calculations

made only reckon them at 650 cubic yards, or 15,600 cubic yards each per month.

A black laborer on the Isthmus is capable of loading an 8 cubic yard car per day, but the average for each for digging and loading is only reckoned $2\frac{2}{3}$ cubic yards per day, or 64 yards per month for each laborer.

The dredges of the Franco-American Trading Co. will average 520 cubic yards per day, or 12,480 cubic yards per month each. French marine dredges, with dumping scows, for deepening channels, will average 65,000 cubic yards per month each. Centrifugal sand and dirt pumps for removing loose materials and discharging through pipes, are estimated at 650 cubic yards per day for each, or 15,600 cubic yards per month.

It is also estimated that each large dirt car will average monthly 292 cubic yards; each small hand car (Decauville system), 130 yards; each transporter, or endless band discharger, 39,000 cubic yards.

Therefore, to obtain a monthly average of 2,600,000 cubic yards of dry excavation, the following are required: 4,500 large dirt cars, 4,000 Decauville hand cars, 20 endless band dischargers.

For dredging, 40 dredges are required, each averaging 650,000 cubic yards per year. With this machinery it is calculated that it will take two years to do the dredging, and three years for the dry excavation; showing that if the dredging should only commence on the 1st of January, 1886, and the dry excavation on the 1st of January, 1885, it would still be possible to open the canal on the 1st of January, 1888. For any unforseen circumitances that may arise, there may be offset all the dry excavations made up to January 1st, 1885, and the dredging up to January 1st, 1886, and one whole year to spare 1888, besides.

To carry out the work to completion by the time specified, 1888, there is required 10,460 cars, 250 locomotives, 44 dredges, 3 hopper barges, 10 hand dumping scows, 34 lighters, 97 portable engines, 100 excavators, 325 pumps, 50 earth elevators, 20 endless band transporters, 56 hoisting apparatuses, 38 steam windlasses, 814 hoisting engines, 260 miles of rails, 4 steamers, 30 towboats, 316 floating apparatuses of different kinds.

Out of this large amount of machinery there has already been sent, or is now on the way to the Isthmus: 72 portable engines, 316 floating apparatuses, 30 towboats, 4 steamboats, 21 dredges, 260 miles of rails, 8,960 cars, 122 locomotives, 814 hoisting engines, 38 steam

windlasses, 56 hoisting apparatuses, 256 pumps, 20 endless band transporters, and 79 excavators. The details of these estimates are too many to be discussed here, but from the fact that Mr. de Lesseps' assertion of the opening of the canal in 1888 is fully endorsed by the Technical Commission, made up of competent engineers attached to the company, it may reasonably be supposed that except in case of unforseen circumstances or accidents this promise will be redeemed. Moreover, it may be said that the opinion of several of our Navy officers, as shown by their reports, after inspecting the works, have expressed confidence in the successful completion of the work. Among them may be mentioned Rear-Admiral Cooper and Lieut. Rodgers, the latter having made two reports, one in 1883 and the other in 1884. Capt. Meade also made a favorable report in March, 1882. Lieut. Kimball, in the *Manchester Guardian,* of March 5, 1888, expresses the opinion that the canal will be finished in three or four years. Commander Selfridge, at the Paris Congress of 1879, expressed his strong confidence in the competence of its members and in their decisions, stating that the United States would accept all their deliberations. He showed his faith in their ability by voting for the Panama route in the fourth, or Technical Commission. When Mr. de Lesseps came to New York in March, 1880, a number of merchants and prominent gentlemen expressed their views freely in favor of the Panama Canal. So that it may be said there is every probability in favor of an early opening of the canal.

CLIMATE AND HEALTH.

On the Isthmus of Panama as in all intertropical countries there are two distinct seasons, the dry and the wet, the "verano" and the "invierno." The first commences about the end of November and the second during the month of May. There is a short cessation of rain, during the solstice of June of about twenty days.

The mean temperature at Colon for the year 1882 has been 79°, at Naos 80°, at Gamboa 58°; the two extremes are at Colon 64° and 94°, at Gamboa 52° and 98½°, at Naos 67° and 94½°.

More rain falls at Colon than any other point and less at Naos. Rains are the most abundant at Colon at the beginning of the rainy season in November, per contra at Naos, on the Pacific, the rains are the most abundant in May or June when the dry season is at hand. Gamboa about midway seems to be more subject to the influence of

the Pacific than of the Atlantic ocean, and the rains are also greatest during May.

The tides at Colon are but slight, varying from $1\frac{1}{2}$ to 2 feet, while at Naos on the Panama side they rise and fall from 16 to 18 feet.

During the dry season the trade winds prevail, but in the rainy season the winds are feeble and variable. The air is always more or less charged with moisture; the climate is therefore debilitating, especially to the whites, but with ordinary precautions in diet and taking care not to be exposed to dampness and especially avoid to keep on wet clothing, ordinary good health can be maintained.

The records of deaths kept by the company show that during June, July and August, 1883, the proportion has been 3 per thousand; from September, 1883, to March, 1884, $5\frac{1}{2}$ per thousand, and for April and May, 1884, again 3 per thousand. The company has provided a full corps of physicians to attend to the workmen and employes distributed all over the line. A fine and large hospital has been put up at Panama; Colon has also a hospital built on the sea-side, small post hospitals are distributed over the line. Nothing has been spared to provide for the health and comfort of the workmen, now numbering about 20,000, mostly blacks from Jamaica, some natives, and men of different nationalities.

The demands for labor has created a strong emigration from Jamaica to the Isthmus, and shiploads upon shiploads of blacks are discharging constantly. These men used to a warm climate resist the debilitating effect of the Isthmian climate better than laborers from northern climates.

Since a falsehood well repeated may be looked upon as truth, it may not be inappropriate to refute again here one of the popular stories told about the insalubrity of the Isthmian climate. It is popularly believed that there is a man buried on the Isthmus for every tie laid on the railroad. To prove the absurdity of this story it is an easy matter to make a calculation. The Panama Railroad is about $47\frac{1}{2}$ miles long, it requires about 74,000 ties for that distance so that according to popular belief there are 74,000 men buried on the Isthmus in consequence of building the railroad. At no time has the railroad company had more than 4,000 men altogether on the railroad works. Modern arithmetic is insufficient to solve this problem.

ORGANIZATION OF THE COMPANY.

The Universal Interoceanic Canal Company has been organized

under the French law for the formation of corporations and copartnerships, dated July 24, 1867, now in existence. This law contains very severe provisions for punishment against maladministration or mismanagement of the Board of Directors and officers of the Company, varying from 500 to 10,000 francs fines, and imprisonment from fifteen days to six months.

Agreeably to this law of July, 1867, Mr. de Lesseps entered articles of incorporation and by-laws before notaries public in Paris, on the 20th of October, which are now in existence and regulate the affairs of the Company.

These articles of incorporation are also in accordance with the requirements of the Law of Concession, No. 28, of May 18, 1878, granting certain privileges for the opening of an interoceanic canal through the Isthmus of Panama, as sanctioned by the Government of the United States of Colombia. This concession was granted to Lieut. Lucien N. B. Wyse, as the representative of the "International Civil Society of the Interoceanic Canal," who sold their rights and privileges to Mr. Ferdinand de Lesseps.

At the time the first subscription was opened it was intended to locate the central office in the country subscribing the largest amount. Subscriptions were opened in several countries of Europe and in the United States. France having subscribed the largest share of the capital, the offices of the Company were opened in Paris, and are now located at No. 46 rue Caumartin, in that city. The President of the Company is Count Ferdinand de Lesseps; the General Secretary, E. Martin. A Board of Directors, of 24 members, has been elected, and Mr. P. Daubrée appointed as Secretary.

The Company is represented in this country by the American Committee, composed of four members, Messrs. R. W. Thompson, ex-Secretary of the Navy, Chairman; Jesse Seligman, of the banking house of J. & W. Seligman & Co.; E. P. Fabbri, of the banking house of Drexel, Morgan & Co; Jno. W. Ellis, of the banking house of Winslow Lanier & Co.; all of New York, and Charles Colné, Secretary of the Committee. Their offices are located in the Mills Building, 15 Broad street, New York.

FINANCIAL.

Since the Canal Company has been organized four subscriptions have been put upon the market, as follows:

No. 1.	In December,	1880, fs. 150,000,000	=	$30,000,000
No. 2.	In September,	1882, fs. 109,375,000	=	21,875,000
No. 3.	In October,	1883, fs. 171,000,000	=	34,200,000
No. 4.	In September,	1884, fs. 105,975,000	=	21,195,000
		Francs 536,350,000		$107,270,000

Loan No. 1, only half paid; the balance of $30,000,000 is subject to call on three months' notice.

Loan No. 2 is full paid.

Loan No. 3 is full paid.

Loan No. 4 is payable in instalments between Sept. 25, 1884, and July 5, 1885.

The financial resources of the Company on the 5th of September last are shown as follows:

Cash on hand.....................	fs. 85,647,424.64
Instalments due on shares....	fs. 147,500,000.00
Loan of September, 1884......	fs. 129,000,000.00
	Francs 361,967,424.64 = $72,393,485

The Canal Company, through their representatives in this country, have disbursed about $22,000,000 for purchase of shares of the Panama Railroad, and in payment of machinery, materials, etc., purchased from our merchants and manufacturers.

Machinery, supplies, etc., are constantly being shipped to Colon; thus this country is deriving daily benefits from this foreign capital invested here.

CONCLUSION.

It is not my purpose here to describe the special and marked advantages that commerce and navigation will derive from the opening of the Panama Canal. I believe it is a well settled belief, here and abroad, that such a channel of navigation has become an absolute necessity. From the fact that two competitors are in the field with the Panama Canal, the proposed Nicaragua Canal and the ship railway by way of Tehuantepec, it would seem that such enterprises must be thought profitable and necessary. It is a question for capitalists to decide whether one canal is sufficient, or whether we must have another by way of Nicaragua and a ship railway besides. The Panama Canal Company, I am sorry to say, has often been misrepresented in this country. Under the pretence that we must have "our own canal" as it is termed, another project has been brought before the people and

urged as an "American Canal." How a canal cut through foreign territory precisely as the Panama Canal is, can be more of an "American Canal" is not exactly clear to the mind. If the fact that American money invested in this enterprise will make it an American Canal, per contra, many of our railroads must be foreign roads, because many of them are built with foreign capital. Yet when this capital was proffered to us from abroad, common sense dictated to us that we should not refuse such a help; the man who would take a different view would be looked upon as of unsound mind. Our railroads are American because they are upon American territory. The Panama or the Nicaragua Canals cannot be American because they are not upon American territory.

The pretence that American money would give an American character to a Canal by the way of Nicaragua can scarcely be maintained in the face of friends of this enterprise making statements that money for its construction would be raised in England without any difficulty.

Upon the engineering difficulties of this route I shall not comment. The decision of the Paris Congress of 1879, I accept as sufficient as Admiral Selfridge did at the time of its meetings. This would seem to be qualified and sufficient authority.

On the other hand it is urged that in case of war the Canal should be controlled by America. All the diplomacy in the world and the fine spun sophistry of diplomatic correspondence does not amount to a feather's weight in case of war. All treaties and so-called solemn obligations are cast to the winds in the face of war. War is brutal and uses brutal means, it is a question for the best ships and best artillery to settle, and not one for pen and ink. It is not worthy of a great nation like ours to invoke such reasons against the prosecution of a maritime canal. It is against our self-interest to oppose it, and if I can find no higher and nobler motives to appeal to, let us at least not be blind to a question affecting our prosperity. I know of no parallel case of opposition to a universally recognized worthy enterprise except it be the inexplicable fear of the English against the boring of a comparatively small mole hole under the channel between France and England. I trust we shall not use that as one of the arguments to strengthen our opposition. How such a small hole can throw into convulsions of fear some of the so-called eminent men of England is beyond the comprehension of the average mind. Mr. de Lesseps, in his past experience in building the Suez Canal, has met with the same opposition from so-called leading men in England. The history of all

great enterprises has been one, in many instances, of continual opposition and positive assertions of failure, from great men who have often lived long enough to discover that their greatness did not lie in that particular line of prediction.

It is puerile for a great nation like ours to take the attitude of the dog in the manger, and invoke trivial and unsound reasons for opposing the completion of one of the enterprises that will outlive the history of our times by many and many years. I have had occasion in my official capacity to ascertain the views of Mr. de Lesseps, the President of the Panama Canal Company, and I say to you as I have said it two years ago in this hall, that whoever represents him as antagonistic to American interest and cherishing anti-American views, deliberately misrepresents him. Mr. de Lesseps is too much of a cosmopolitan to entertain the narrow views of the intensely national but misguided citizen, who can see nothing beyond the boundaries of his native land. If he were not actuated as he is, by broad and liberal views, there would still remain a question, which with us is seldom if ever overlooked, that of self-interest; for it is well known that the United States will be the best customer of the interoceanic Canal.

Moreover, have we not a sufficient guarantee that the Panama Canal is not being built as an antagonistic enterprise to American interest, when we find such men as the Hon. R. W. Thompson, the ex-Secretary of the Navy, connecting himself with the Company as a representative of American interests? The same may be said of the members of the American Committee, gentlemen well known for their national feelings. Is it reasonable to suppose that men of their reputation would attach their names to any enterprise having the least flavor of anti-Americanism?

However, I have a better opinion of my fellow-citizens than to suppose that this uncalled for spirit of opposition extends to any distance beyond the limits of a few interested patriots. I have confidence in American intelligence and American fair play, the outgrowth of our liberal and free institutions, and no man nor any set of men can hope to mislead the people for any length of time. We think and act for ourselves, and the public mind is the indication for leaders. If we ever travel in the wrong path we are not slow to discover it and change our course; if we unintentionally do wrong we are not ashamed to acknowledge it and repair the damage. Were it otherwise it would not be American.

GLIMPSES OF THE INTERNATIONAL ELECTRICAL EXHIBITION.

By Prof. E. J. Houston.

Comprising the substance of remarks made by request, at the Stated Meeting of the Institute held, Wednesday, October 15, 1884.

No. 1.—Telephoning Without Wires.

Professor Houston remarked:—After consultation with our President, I have concluded that it will not be advisable in the limited time of a single meeting to attempt to discuss all the topics mentioned since that would be more than can comfortably be completed. I will, therefore, confine myself this evening to a few remarks on the subject of telephony. I wish, however, to be distinctly understood that I do not mean to attempt to describe all the varieties of telephones exhibited at the Exhibition, since that would take longer than the time at our disposal. I wish rather to call your attention to a few of the many novelties in telephony which were presented by the various exhibits, and I believe that we can find abundant material if we consider some of the apparatus exhibited by Prof. Alexander Graham Bell, and by Prof. Amos E. Dolbear.

The mechanism of the magneto-electric telephone is so generally understood that I need not stop to describe it. This species of telephone, as you are well aware, is a contrivance by means of which speech is readily transformed into electricity, the electricity so produced is transmitted over a conductor, and transformed at the other end of the conductor into speech. The telephone, therefore, is practically a dynamo-electric machine in which the steam engine for driving the same is replaced by the voice. The electricity so produced traverses the conducting wire and entering a second dynamo, which acts as a motor, produces motions in a diaphragm which result in the reproduction of the original speech.

Without going into any further description of the telephone permit me to call your attention to some very beautiful experiments conducted by Prof. Bell, which resulted in the discovery by him that the conducting wire, usually employed for transmitting the electric current, could be replaced by a beam of light. This discovery resulted in the invention of an apparatus called by Bell the *photophone*, and now

generally known under the name of the radiaphone. Noticing a photophone on exhibit in the collection of the Bell Telephone Company, I prevailed on the gentleman in charge of the exhibit to bring the instrument to the Institute, so that I might have the pleasure of showing it to the members and explaining the method by which it is operated.

Before doing so, however, it may be well to briefly review what has been done with apparatus of this character, and in this direction I cannot do better than to call your attention to an elaborate series of experiments conducted by Prof. Bell in connection with Mr. Sumner Tainter, an account of which is published in a paper read before the American Association for the Advancement of Science, in Boston, August 7, 1880.

The experiments carried on by these gentlemen were originally made with a view of studying the causes of the curious sounds emitted when a vibratory beam of light was permitted to fall on certain substances. These experiments were first carried on with the then rare element selenium, but they were afterward extended to other materials which were found to produce the same phenomena. Among some of the substances which they found would emit sounds when vibratory beams of light were permitted to fall on them are gold, silver, platinum, iron, zinc, lead, copper, hard rubber, celluloid, gutta percha, ivory, paper, and wood.

In order to produce these effects, alternations of light and darkness, following each other with a certain rapidity, were permitted to fall on plates of the substances named, when musical sounds were emitted, the pitch of which was dependent on the rapidity with which the light and shadow succeeded one another. By properly modifying these alternations of light and shadow, even articulate speech was thus obtained.

It is not, however, to this method of causing light to produce sound that I wish to call your attention, but rather to the possibility of using a beam of light in the place of the conducting wire ordinarily used in telephonic communication. In order the better to do this it may be well to give some little attention to the properties of selenium, as this is the substance now used in connection with Mr. Bell's photophone.

Selenium was discovered in 1817, by Berzelius, while conducting a series of experiments on the refuse of some sulphuric acid works. Noticing a peculiar odor emitted by the refuse and ascribing it to the

then rare metal tellurium, he endeavored to separate this element from the refuse. Without asking your attention to the very excellent work he performed in this connection, I will simply mention that as the result of a series of elaborate experiments made on the refuse, he obtained, not the element tellurium for which he was seeking, but an entirely new element which he named selenium.

Selenium, as obtained by Berzelius, is a non-conductor of electricity. There are, however, different forms in which it can be obtained. If selenium is rapidly cooled from a fused state, a form known as the "vitreous variety" is obtained. This variety has a dark brown color, when in thin films, is transparent to ruby red light, and is a non-conductor of electricity. When, however, fused selenium is slowly cooled, a variety known as "crystalline," or "granular selenium" is obtained. This is opaque to light, is of a dull-lead color, and is a conductor of electricity.

The conducting power of selenium is, however, exceedingly slight, its electric resistance as compared to that of ordinary metals being enormous. It was the fact of its great electric resistance that induced Willoughby Smith to employ selenium in his system of cable testing and signaling during submersion, for the high resistance at the shore end of a submarine cable. When so employed, phenomena were observed, which eventually led to the invention by Mr. Bell of the photophone.

Although the introduction of the selenium resistance at the shore end of the cable readily afforded the high, artificial resistance required, yet Mr. Smith was exceedingly puzzled to find that its value was subject to remarkable fluctuations. Patiently investigating the cause of these variations of resistance, he at last discovered the exceedingly curious fact that the electric resistance of selenium is much less in the light than in the dark. He announced this discovery to the Society of Telegraph Engineers on the 17th of February, 1873. As might naturally be expected, this peculiar property of selenium was at once investigated by scientific men in different parts of the world, and several varieties of selenium, varying very greatly in their conducting power for electricity in the light and in the dark were obtained. For example, in February, 1876, Dr. C. W. Siemens, obtained a variety of selenium whose conductivity was fifteen times as great in the sunlight as in the dark.

Previous to the investigations of Bell and Tainter the variations

in the electric resistance of selenium by light were shown by the use of a galvanometer inserted in the battery circuit in which the selenium was placed. As long as the electric resistance remained constant the needle was motionless, but when light was flashed on the selenium, since a greater electric current then traversed the circuit, the needle of the galvanometer was at once deflected. Now, when Prof. Bell began his investigations on selenium, it occurred to him to replace the galvanometer by a telephone. From his knowledge of this latter instrument he readily appreciated the fact that in order to obtain its greatest sensitiveness, it would be necessary to cause a very quick succession of variations in the intensity of light to fall on the selenium; for, in the magneto-electric telephone it is only at the moment of change in the intensity of the current that any audible effect is produced by the diaphragm. He therefore rapidly varied the alternations of light and shadow by permitting an intermitting beam of light to fall on the selenium resistance. Under these circumstances a musical note was heard, the pitch of which was dependent on the rapidity with which the variations in the intensity of light followed one another. These experiments enabled him to announce on the 17th of May, 1878, in a lecture delivered at the Royal Institution of Great Britain, the possibility of hearing the fall of a shadow.

From these experiments the idea naturally suggested itself to him of employing a beam of light in place of the conducting wire ordinarily employed in telephony.

Now what must we have in order to apply the principles already explained as to the variations in the electric resistance of selenium by the action of light, to permit us to talk along a beam of light? We need a beam of light to replace the conducting wire. We need an arrangement by which this beam of light shall be varied in its intensity by the action of the voice, and a contrivance by which the beam so varied shall be permitted to fall on the surface of a selenium resistance which is included in the circuit of a voltaic battery and a telephone. Under these circumstances a person talking against an apparatus which we will subsequently explain, causes rapid variations in the intensity of the beam of light. These variations being imparted to the beam, produce corresponding variations in the amount of current that flows through the circuit. These in their turn produce in the diaphragm

of the telephone, movements, which are translated by the ear of the observer into articulate speech.

The most important part of a photophone is evidently the selenium resistance. These resistances are generally made in the form of what is known as selenium cells. Previous to the time of Bell and Tainter these cells were not in a condition suitable for use in connection with an ordinary telephone. The least resistance of any selenium cell being, I believe, about 250,000 ohms in the dark. Such cells, of course, could not be used in connection with a telephone. Messrs. Bell and Tainter, however, succeeded in making cells whose resistance is about 300 ohms in the dark, and about 150 in the light. It is such cells that are employed by them in connection with their system of radiaphony.

Messrs. Bell and Tainter attribute their success in lowering the resistance of their selenium cells to the use of substances like brass that exert a slight chemical action on the selenium. This action, in the opinion of Mr. Bell, prevents the selenium from acting towards other substances somewhat like greasy water does, and so ensures the contact of an extended surface instead of a series of minute contacts. The selenium cell we will employ this evening is formed of alternate metallic discs of brass separated by discs of mica, of slightly smaller diameter. The spaces between the brass discs over the mica, being filled with selenium. The alternate brass discs are connected together, as are also the selenium discs; that is to say, the selenium cell is coupled in multiple-arc.

Let us now inquire what must be done to the beam of light in order to permit it to be suitably varied in intensity by the action of the voice. As I understand Mr. Bell's invention in the art of articulate rádiaphony, it consists in the use of an undulatory beam of light in distinction from a vibratory beam. He claims, I believe, that it is not possible to transmit articulate speech by means of a pulsatory beam of light, that is by means of alternations of light and absolute darkness. What he does is to produce variations in the intensity of the light that correspond with the variations in the amplitude of the sound waves produced by the movements of the plates of the diaphragm. This he accomplishes as follows: a parallel beam of light is permitted to fall on a flat plate of thin glass which is covered with a film of bright metallic silver. This plate is fixed at its edges in a manner similar to the diaphragm of the telephone. If, now, a speaker talks

against the back of the plate, the sound waves set it into vibration and cause it to become alternately convex and concave. These changes, you will readily understand, will result in alternately, causing the parallel rays of light in the beam to diverge and converge and thus to illumine the selenium cell more faintly or more brightly, but at no time to cut off all the light from it. In other words, the effect of the voice of the speaker against the plane silvered reflector is to produce undulatory, photometric variations in the beam of light that falls on the selenium resistance.

In order that you may be able to see these variations in the photometric intensity, I will illumine a cloud of smoke by means of a parallel beam of light. When now the voice is permitted to fall against the plate you will observe the very pronounced manner in which the breadth of the beam is altered.

Mr. Wilson, of the Bell Telephone Company, who had charge of the Bell exhibit at the Exhibition, has kindly consented to operate the apparatus for me, and I will now show you in actual operation, the process of talking along a beam of light. The limits of our lecture-room would naturally prevent a fair trial as to the success of the experiment, since any remark a speaker made at one end of the beam of light could be distinctly heard across the air space by the observer at the telephone. I have, therefore, connected the telephone by means of a metallic circuit with a room on the floor above, and an observer in that room will be able to hear all that is spoken against the plate in the lecture-room.

The light we will use for this purpose is the lime light. Arranging the lenses of the lantern so as to obtain a parallel beam of light, I allow it to fall on the plane silvered mirror before described. When, now, we talk against the plate, the beam of light is caused to vary in the manner we now see, and this undulatory beam falling on the selenium pile, which as we see is placed at the focus of a parabolic reflector, produces corresponding variations in the current that traverses the circuit of the battery and so permits articulate speech to be reproduced in the telephone.

Another experiment in telephony made at the exhibition may not be devoid of interest. I allude to the experiment tried by Professor Dolbear with his ingeniously constructed electro-static telephone. This instrument, as you are aware, is not magnetic in its action, and works on a principle entirely distinct from that of the magneto-electric

telephone, the vibrations of the diaphragm being caused by the attractions and repulsions produced in two parallel conducting plates. The peculiarity of this experiment consists in the fact that with this instrument we can telephone, not only without wires, but without even a beam of light. Holding a telephone to the ear and having its terminals not connected with any metallic conductor at all, we can walk around a room and yet in all positions hear what a person is saying, who is talking into the telephone at the other end of the line. When I speak of a room, however, you will understand that I refer to a room of the size of that occupied by Professor Dolbear at the Exhibition, which was about eight feet square.

If you regard this experiment as being somewhat incredible, I can assure you of its truth for I have tried this experiment myself. The phenomenon, however, is difficult to understand; indeed, like many other surprising things in science, it is difficult to explain why such an experiment was not tried sooner, but as we all know, it is those very things that are apparently so simple that require the greatest ingenuity to originate.

The explanation of the phenomenon, as I understand it, would appear to be somewhat as follows: One of the plates already referred to, being connected through the body of the observer to the ground, is thus joined to one end of the telephone circuit; the other plate is connected to the other end of the circuit by a line of polarized air particles. The experiment is simply an exceptional application of the principles of electro-static induction, and I am not at all sure but what it may be susceptible of a great increase in delicacy and thus become of considerable commercial value.

Mr. Outerbridge remarked:—The very interesting explanation of the photophone is exceedingly clear in every respect, and I think every one in this building will understand just exactly the method by which the action of a beam of light carries sound, but at the beginning there was a statement made which Professor Houston may have made inadvertently, or it may have been a mere *lapsus linguæ,* and I should be glad to call it to his attention. Professor Houston said a beam of light falling upon a selenium cell, or on other metals would cause a sound. Is it actually so?

Professor Houston:—The statement I have made concerning the

audible sounds which selenium and other substances emit when a vibratory beam of light is allowed to fall on them, are vouched for by Professor Bell, and will be found in a paper which he read before the American Association for the Advancement of Science, in Boston, in August, 1880. He found that when plates of these substances were simply held to the ear and light was permitted to fall on them that sounds were distinctly heard. I believe he found that these phenomena were more marked in the case of thin plates of the materials and ascribed the cause of this circumstance to the fact that the action of the light was a surface action.

Dr. Wahl:—I would like to ask Professor Houston if any success has been made in interpreting the sounds transmitted by the photophone?

Professor Houston:—It is not all that one might desire, though I could understand, I suppose, all that was spoken; but you will understand that the photophone is an exceedingly delicate apparatus, and in the hurried and necessarily clumsy way in which we have it arranged, we cannot expect to be able to hear everything distinctly, but I could distinctly understand words and distinguish musical sounds.

Electric Light in Theatres.—M. Brandt places alternately in a continuous line, forty lamps of ordinary glass, forty of green glass, and forty of red glass, making a hundred and twenty lamps in all, at the foot of the stage. Each series of forty lamps forms a separate circuit. The three series can be lighted independently, or they may be combined, in order to obtain different effects of color. For example, a delicate rose hue may be produced by simultaneously lighting the red and the white lamps; a moonlight effect, by a combination of the white and the green lamps. In order to pass gradually from the latter to full daylight, it is only necessary to increase the resistance in the green circuit while strengthening the current in the white lamps. Moreover, the two sides of the stage may be lighted independently, because the 120 lamps are again sub-divided into two circuits of sixty each. We may thus have a moonlight on one side of the stage, while the other side, at the moment when an actor enters with a torch in his hand, seems to be illuminated by the reflection from the torch. When the footlights are of gas, a current of hot air ascends above the whole line of lights, forming a sort of gaseous wall between the stage and the audience, which often makes it difficult to hear the actors. This inconvenience is suppressed by electric lighting, and the opera singers are agreeably surprised at the great improvement.—*Lumière Electr.*, May 24, 1884. C.

CORRESPONDENCE.

AN ITEM OF HISTORY, AS TO THE IDEA OF MAKING THE PARTS OF GUNS INTERCHANGEABLE.

Committee on Publication, Journal of the Franklin Institute:

It has become in some sort customary for writers in American newspapers and magazines to claim as a distinctively American contrivance the constructing of machines and implements, and particularly firearms, with interchangeable parts ; that is to say, the making of each of the parts of any one machine so precisely like the corresponding part of every other that it will fit one machine as well as another, and in case of such substitution can serve its proper purpose as well as the part which it has replaced. Recently, Mr. Charles H. Fitch, in an apparently carefully prepared paper entitled "The Rise of a Mechanical Ideal," which was published in the *Magazine of American History*, for June, 1884 (page 516), while admitting that "the plan of uniformity in firearms was attempted in France in 1783, and was noticed by Thomas Jefferson . . . . who advised the purchase by the United States Government of French arms having the feature of uniformity, but nothing came of it ;" and urging, with evident justice, that "we find in the history of this manufacture a series of men who were imbued with the idea and pushed it to successive degrees of mechanical perfection ;" has said explicitly, "first of these was Whitney, inventor of the cotton gin, who introduced some of its most essential administrative features at his armory at Whitneyville, Connecticut, which was established before the close of the last century. Next, Hall invented a breech-loader, designed with especial reference to its interchangeable manufacture. . . . etc."

I would submit, that as a mere matter of historic fact, and solely as concerns the question of priority, it may possibly be open to doubt, whether the foregoing statement can be made to harmonize with the following remark of Professor Erman who, traveling into Siberia in 1828, wrote as follows :* "Leo Sobakin (born in 1742) was a serf in the Government of Tver. He early displayed great mechanical ingenuity and was sent to England to study. On his return to Russia he was appointed superintendent of the machinery at Ije. Of the apparatus erected or brought into use under his direction, much was undoubtedly invented by himself, and was quite original. It conduced much, for example, to the perfection of the firearms made at Ije that every piece was made there according to a model, and if it did not exactly fit or correspond with that model was rejected by the overseer. The system of shaping the several parts by simple pressure was also carried to a great extent. All the pieces of the same denomination in Russian arms are consequently so perfectly equal or alike that the experiment has often been successfully made of taking to pieces a large number of muskets and then from the promiscuous heaps of similar parts to put them all together again."

F. H. STORER.

BUSSEY INSTITUTION, September, 1884.

* See the 172d page of the first volume of his "Travels in Siberia," *i. e.*, the English translation of his "Reise um die Welt durch Nord-Asien."

Committee on Publication, Journal of the Franklin Institute:

GENTLEMEN:—In a recent issue of your JOURNAL a writer over the initials "H. B." gives a notice of my "Treatise on Toothed Gearing," which seems to me to be unfair and erroneous; I trust you will therefore allow me brief column space in which to defend my little work. Figure 12 in my book (the figure to which "H. B." takes most violent exception) was drawn without regard to accurate shape of the tooth profiles, and the first point of contact between the teeth shown was unfortunately drawn so close to the pitch point that the engraver carelessly made the points coincide. In my original drawing, which is before me, these points do not coincide, and there are arcs drawn through both points instead of the one shown in the book. The small rolling circles—which should be dotted—are assumed circles and are drawn small because the generating circles for cycloidal profiles are, in practice, always smaller than the pitch circles. The two points of contact between two working teeth mentioned by "H. B." are on separate teeth and are so explained in the book. I passed over the subject of "interchangeable gears" briefly in order to save space and because I believed the small space given to it sufficient for a general understanding of the subject. I make use of the circles at the bottoms of involute teeth as the evolute circles because I believe it best; it gives stronger forms and, according to my experience, as accurate profiles. The statement that radial flanks are used in involute teeth is *not* incorrect.

The teeth of my Figure 66 were drawn off-hand with a common pen, and are not supposed to be accurate, since the figure simply illustrates a principle. I notice that Reuleaux, in his "Constructeur," gives a precisely similar sketch, and states that the contrivance is in actual use in an astronomical clock at Prague.

Notwithstanding the fact that my Figure 100 was drawn hastily with the compasses, and not "designed," if "H. B." will measure the peripheries of the gears there represented by any more accurate method than that of "placing the square wheel inside of its mate," he will find that they are very nearly equal, although this fact has no important connection with the method illustrated by the figure. I believe that practical machinists and artisans, for whom the book was written, will be in no danger of becoming "hopelessly confused," but will find my Treatise as simple, and clearly explained as the complicated nature of the subject will allow.

J. HOWARD CROMWELL.

Committee on Publication, Journal of the Franklin Institute:

GENTLEMEN:—Allow me to answer Mr. Bilgram's communication to you, published in the October number of the JOURNAL, page 307, in regard to "Tests by Hydrostatic Pressure."

The quoted statement was not made by me at the meeting of the Institute, but added the same when I read the proof, where I first noticed Mr. Bilgram's remarks upon the force of impact in connection with velocity of sound, etc.

The question of force of impact which I elucidated at the meeting of the Institute, is based upon well-known and established physical laws, which are

abundantly confirmed in practice, and for which laws I am not responsible. Mr. Bilgram sets himself above, and defies the laws of nature in saying that "The alleged pressure resulting from impact is simply another instance of the anomalies at which theorists arrive." The "theorist" in this case is the Creator of the universe, who has established these laws, and to whom I refer Mr. Bilgram for redress of his grievances. It is not my fault that these laws do not agree with Mr. Bilgram's notions of what they ought to be. Mr. Bilgram says that "the principal factor in the phenomenon is friction of water in the pipe." Mr. Bilgram is in error about the friction being the principal factor in the phenomenon. Under the very high pressure in a hydrostatic test, the friction is of no importance.

JOHN W. NYSTROM.

PHILADELPHIA, October 18, 1884.

OBITUARY.

ROBERT EMPIE ROGERS.

Robert Empie Rogers, the subject of this sketch, was the son of the late Dr. Patrick K. Rogers, of Philadelphia, and was born, in the year 1814, in the city of Baltimore. He was one of a notable brotherhood, all now deceased, who were distinguished for their scientific attainments. His eldest brother, James B. Rogers, successively occupied the chair of Professor of Chemistry in the Washington Medical College at Baltimore, the Medical College of Cincinnati and the University of Pennsylvania. Henry D. Rogers, another brother, was a noted geologist, best known, perhaps, by his great work on the Geology of Pennsylvania. William B. Rogers, still another brother, whose death occurred two years ago, was the President of the Massachusetts Institute of Technology.

Robert E. Rogers was educated at the University of Pennsylvania, and, after his graduation, turned his attention especially to the study of chemistry and toxicology, as the assistant of Prof. Robert Hare. The earliest scientific work in which he was engaged was in connection with the Geological Survey of Pennsylvania (conducted by his brother, Henry D. Rogers), on the official staff of which he served as chemist in the campaigns of 1837 and 1838. In 1844 he was elected to the chair of Chemistry in the University of Virginia, from which he withdrew in 1852 to occupy the same chair in the Medical Department of the University of Pennsylvania, made vacant by the death of his brother James. Dr. Rogers held this post for twenty-five years, during a considerable portion of which he was the dean of the faculty.

In May, 1877 he resigned his position in the University of Pennsylvania to accept the chair of Medical Chemistry and Toxicology in the Jefferson Medical College, a position which he continued to occupy to within a few weeks of his death.

Dr. Rogers' connection with the Franklin Institute began in 1852, in

which year he was elected a member, and this association, which was most active, useful and honorable, continued uninterruptedly until terminated by his death. In 1855 he became a life member. In 1857 he was elected to the Board of Managers, in which he served until the following year, when he was chosen Vice-President. This office he occupied continuously for a period of eighteen years, and in the year 1875 was elected to the highest office in the gift of the Institute, that of President. This post of honor he filled for four years, from 1875 to 1878 inclusive. In 1879, declining a re-election, he was again elected to the Board of Managers, of which he was a member at the time of his death.

During most of the thirty-three years of his connection with the Institute, Dr. Rogers was prominently identified with its work. He was for many years an active member of the Committee on Instruction, and delivered several courses of lectures before the Institute on chemistry, electricity and kindred subjects.

He served also upon numerous special committees, the most notable of which were that engaged in the tests of the efficiency of dynamo-electric machines and the committee to investigate the dangers of electric lighting. Of both these committees he was the chairman, and the results of their work, which are recorded in the JOURNAL, were highly creditable. The investigation of the comparative efficiency of the dynamo-electric machine, indeed, was the first that had been made, and the work of the committee has a permanent value.

As a teacher and lecturer, Dr. Rogers had eminent qualifications. He had thorough command of his knowledge, and could easily and quickly avail himself of it. He was a fluent and eloquent speaker, and by the dignity and impressiveness of his manner, the elegance of his diction, his clearness of statement and his admirable skill as an experimentalist, he enjoyed a rare degree of popularity. The announcement that he was to lecture was sufficient to crowd the lecture-room of the Institute to the limit of its capacity, and many, doubtless, who read this tribute to his memory, will recall with pleasure the interest and enthusiasm which his presentation of a subject was sure to excite.

Dr. Rogers was a mechanic of no mean attainments, and made a number of inventions, the most notable of which, perhaps, is the form of steam generator known as the "Rogers & Black Boiler."

He was the author of a number of medical treatises, and contributed many papers on medical subjects to the scientific journals. His most recent literary work was the editing of an American reprint of Lehman's Physiological Chemistry.

In his intercourse with others, Dr. Rogers was distinguished by an unvarying affability and courtesy of manner; and they who enjoyed his friendship and intimacy, will cherish his memory as that of a most amiable, genial and accomplished gentleman.

At the time of his death, which occurred on the 6th of September, 1884, Dr. Rogers was in his 71st year.

J. E. MITCHELL,	CHAS. A. CRLSSON, M. D.
EDWIN J. HOUSTON,	GEO. M. WARD, M. D.,
ISAAC NORRIS, M. D.,	WILLIAM H. WAHL., PH. G.

Franklin Institute.

[*Proceedings of the Stated Meeting, held Wednesday, October* 15, 1884.]

HALL OF THE INSTITUTE, October 15, 1884.

The President, Mr. William P. Tatham, in the Chair.

Present, 197 members, and 31 visitors.

The President, upon calling the meeting to order, remarked : "I desire to say, on behalf of the Managers of the Electrical Exhibition, which was closed last Saturday without ceremony, that we express our gratitude to the Almighty, that the prosperity which was invoked at the opening of the exhibition was continued to the close. The chairman of the general committee is present, and, no doubt, will be glad to make a verbal and necessarily imperfect report of the results of that exhibition. It will be his duty hereafter to make a detailed report to the Board of Managers. This will be published, and he will then have the pleasure of giving the credit which is due to everyone who has been concerned in the exhibition and contributed to its success. In case he, with his characteristic modesty, should omit to mention himself, I desire to mention his part myself.

"Col. Banes has been the chairman of the standing 'Committee on Exhibitions' for a number of years, during which he had only to watch his opportunity. This opportunity at last arrived and the Board of Managers then charged him with the care of the exhibition, and clothed him with adequate and corresponding power; and I must say that he has exerted those powers with excellent judgment, moderation and success. His clearness of perception and his energy in direction, his quickness of decision and execution, and his admirable judgment, have made him the very model of a manager for the electrical exhibition."

Col. Banes remarked :—"Mr. Chairman, I desire to thank you for the very kind and flattering words that you have said. Permit me to say through you, to the members of the Institute, that any report that I can make at this time would, of course, be exceedingly imperfect. In the first place we have not by any means closed our financial affairs, and certain very important matters yet in progress will probably require two or three weeks before they are settled.

"The exhibition has been successful, as you all know, from more points of view than one. Some few months ago when I had the honor to make the motion that we should have this exhibition, a gentleman whom I respect very highly said to me 'Are you going to take a leap in the dark?' but I felt that the Franklin Institute had the reputation to make it a success, and that we were close to success in this enterprise. From the very first I do not think there has been any doubt of it. We did not expect at all that we would make any profit, but there has been such a generous spirit shown by the members of the Institute and by the public generally, that we are able to report that, after paying for the building, shafting, steam power, etc., there will probably remain a balance of about $10,000.

"I desire to say a few words in connection with the progress of the enterprise: In the first place, it was early determined that this exhibition should be very largely educational, and, for this reason, the Committee determined on publishing a little paper called the 'Bulletin of the International Electrical Exhibition,' which some of you have seen. This paper has been published every two weeks, and I speak of it for the reason that quite a number of gentlemen connected with the Institute have written for it, and as it was determined to distribute it as largely as possible, we have kept no duplicates on hand.

After the paper was started, it was determined to bring the exhibition to the attention of the schools, and the gentlemen of the Board of Public Education decided to permit the pupils of the Central High School, the Normal School, and the Grammar Schools to attend the exhibition. To make these visits profitable to them we succeeded in arranging for guides to describe to them the instruments and electrical machines, and, by way of greater encouragement, offered prizes for compositions. This has excited great competition and the prizes will soon be awarded to the successful ones.

"In addition to the public schools of this city, there were sixty-five from outside of Philadelphia that visited the exhibition, some coming from long distances and some remaining more than a day.

"In addition to this, a series of 'Electrical Primers' was prepared, giving in simple language explanations of the principal electrical appliances. These primers were offered for sale at a nominal price, and had an enormous sale. Arrangements were also made with a number of eminent gentlemen to deliver lectures on subjects relating, more or less directly, to electricity. These lectures were held in the lecture room of the exhibition on two evenings of the week during the continuance of the exhibition, and attracted large audiences.

"The total number of visitors was about 290,000. Of these visitors, $22\frac{1}{2}$ per cent. were what is called complimentary, that is, members of the Institute and those to whom complimentary tickets were given for various reasons. The largest attendance was, I think, on October 7th, when we had 17,047. The smallest attendance was on September 3d, when we had 2,830. The average attendance during the entire exhibition was 8,507.

"I desire also to call your attention to the fact, that we were greatly aided in adding to the success of the exhibition by numbers of scientific gentlemen and professors from different parts of the country, who not only loaned their instruments for exhibition, but also loaned them for the use of the 'Committee on Tests and Measurements,' which Committee is still at work, is doing good work, and will be occupied for three or four weeks longer. We are also exceedingly obliged to the Government of Canada and to our own Government for the aid they have afforded.

"I desire to say in concluding, that I hope that a motion will be offered this evening authorizing and directing our worthy secretary to extend to these departments and to the various gentlemen who have so kindly aided in the Exhibition, the thanks of the Institute."

At the close of Colonel Banes' remarks, Mr. Hector Orr, seconded by

Mr. C. Chabot, moved a vote of thanks to all who were engaged in conducting the Electrical Exhibition. The motion was carried unanimously.

The Special Committee appointed to prepare a memorial of the late Dr. Robert E. Rogers presented a report, which was adopted, and referred to the Committee on Publication. [This report appears elsewhere in the Journal.]

Professor E. J. Houston, by invitation, made some remarks on the telephones shown at the late Exhibition. [An abstract of Professor Houston's remarks appears elsewhere in the Journal.]

The following amendment to the By-Lays of the Institute was offered by Mr. J. D. Rice and seconded by Mr. J. B. Burleigh, viz.:

Amend Article III Section 1, by adding thereto as follows:

"Any member having paid annual dues to the amount of one hundred dollars is entitled to life membership with all the privileges attached thereto."

After considerable debate, the whole matter was postponed until the stated meeting in November.

The Secretary's report embraced remarks on the Cable Railway about being introduced in Philadelphia, the navigable balloon of M.M. Renard Krebs, and on several pernicious forms of so-called lightning conductors, said to be largely introduced in certain of the Western States.

Adjourned.

William H. Wahl, *Secretary.*

Electricity in Coining.—In spite of the greatest possible care, many of the pieces which are prepared for coins are either too light or too heavy. The first are remelted and the others are filed away until they have the proper weight. These operations cause the loss of valuable material, and interfere with the sharpness of the impression. W. F. Chandler Roberts, chemist of the London mint, employed an electric current in connection with suitable acid baths, in order to regulate the solution of superfluous metal, having ascertained that the quantity dissolved was exactly proportioned to the time, if the current was kept constant. He also provided, in a similar way, for the galvanic deposit of additional metal upon coins which were too light. In both methods provision was made for automatically breaking the circuit, when the right weight was reached. These processes cannot be applied at the London mint, where the law directs that every defective piece, whether too heavy or too light, shall be remelted; they have, however, been used with great advantage at the mints of Bombay and Calcutta.—*Les Mondes*, May 31, 1884. C.

Variations of Bismuth in a Magnetic Field.—In a note presented to the French Academy, at its sitting of May 19, 1884, M. Hurion cited recent experiments and researches of Leduc, Kerr and Righi, together with his own experiments which are still in progress, to show that the electric resistance of bismuth increases when it is placed in a magnetic field.—*Lumiere Electr.*, May 31, 1884. C.

INFLUENCE OF TEMPERATURE ON SPECTRAL RAYS.—Fievey has shown that the broadening of the spectral lines in hydrogen, nitrogen, etc., is independent of pressure, and corresponds with elevation of temperature. By recent experiments he has proved, in opposition to the declaration of Van Monckhoven, that pressure has no *direct* influence upon the breadth of the hydrogen lines, but the broadening is due to the resistance which is opposed to the current by the medium which it traverses. Hence he concludes that an increase of complexity in the constitution of a spectral ray is a sure index of an increase of temperature in the producing vapor. This conclusion justifies the inference that the temperature of the solar spots is greater than that of the limb.—*Bull. de l'Acad. Belg.*, No. 4, 1884. C.

OPTICAL MEASUREMENT OF ELECTRICAL CURRENTS.—Since 1878 Henri Becquerel has measured the absolute intensity of a magnetic or electromagnetic field by observing the rotation of the plane of polarization in the light traversing a body placed in the field. The indications of the apparatus are instantaneous; the optical measurement is easily made with great precision; an apparatus for absolute measurement can be easily constructed, and the method can be applied to very weak as well as very strong currents. A description of the method and of the theory on which it is based is given in *La Lumière Electrique*, May 31, 1884. C.

ATOMIC MOTION.—The law of atomic movement is expressed by the formula, $\frac{M v^2}{2} = \frac{4}{3}\pi\rho^3 Pg$, in which M represents the sum of the atomic masses in the molecule; v, the velocity upon the surface of a molecular sphere with a radius ρ; P, the external pressure; g, the constant of gravitating acceleration. Langlois deduces the same formula from the hypothesis that the molecular sphere is formed of thin layers of ætherial molecules which are identical with the ponderable molecules and serve for the transmission of external forces.—*Poggsndorff*, *Beiblätter*, No. 5, 1884. C.

VARIABILITY OF SPECTRA.—In 1882 Stas showed Lt.-Gen. Liagre that, on examining the internal cone of a gas flame which was properly supplied with pure oxygen, spectra would be shown, by the same spectroscope, which were sensibly different in the number of lines, according as the observation was made at the summit of the internal cone, where the temperature is greatest and is sufficient to maintain iridium in fusion, or on the front, or on the side of the internal cone. He also showed that, if a direct spectroscope of small dispersion was used, the spectrum resembled the cometary band-spectrum; but in employing an instrument of greater dispersive power, the bands become lines, of various widths and very sharply defined.—*Bull. de l'Acad. Belg.*, No. 4, 1884. C.

ACOUSTIC EXPERIMENT.—M. Fouchs describes a curious little acoustic experiment. Having closed his ears so as completely to exclude all sounds, and then having conducted, by means of an acoustic tube, the voice of a speaker into his own mouth, he found that the *timbre* was entirely changed so that it ceased to be recognizable.—*La Nature*, May 31, 1884. C.

PREDICTIONS FROM SCINTILLATION.—Montigny has remarked the special predominance of the blue tint in the twinkling of stars, on the approach and during the continuance of rain. On June 2d, 1883, he predicted that the amount of rain during the year would be less than in the immediately preceding years, because the blue was less marked, and the green, which had characterized the fine weather of 1870 to 1876 was more frequent. The prediction was fully verified. For similar reasons he predicts a less frequency and less abundance of rain in 1884 than during the six years anterior to 1883, and he thinks that we are entering upon a series of finer weather.—*Bull. de l'Acad. Belg.*, No. 4, 1884. C.

A NATURAL BAROMETER.—The natives of the Chiloe Islands have a singular barometer, in the shell of a crab which is very sensitive to atmospheric changes It is almost white when the weather is dry ; it becomes spotted with small red points in damp weather and it becomes completely red when rain is falling. The accuracy of the indications has been confirmed by members of the Belgium Mission, who were sent to Chili to observe the transit of Venus and who brought back specimens of the shell.—*Ciel et Terre ; Les Mondes*, April 19, 1884. C.

HYGIENIC COMPARISON OF GAS AND ELECTRIC LIGHT.—Experiments have been made at the Theatre Royal of Munich, in order to determine the elevation of temperature and the amount of carbonic acid, under illumination by gas and by the electric light. Before the play, at the time of the experiments, when there were no more than ten or fifteen persons in the building, the curtain was raised and all the lamps were allowed to burn for an hour. The temperature was observed, at intervals of five minutes, simultaneously in the parquet, in the balcony and in the third gallery. During the plays, when there were on an average between 500 and 600 persons in the theatre, the thermometer was observed every ten minutes. The experiments showed that the electric light greatly diminishes the increase of temperature. It does not render ventilation superfluous, but it requires a less active ventilation than gas since it does not, like gas, contribute to the carbonic acid and to the increase of heat.—*Centralblatt für Elektrotechnik ; L'Electricien*, May 1, 1884. C.

LUMINOUS POWER OF INCANDESCENT LAMPS.—Preece's experiments show that the luminous power of an incandescent lamp increases as the sixth power of the intensity of the current. Since the energy expended increases only as the square of the intensity (or even less if we take account of the diminution in the resistance of the filament when the temperature increases) it follows that the luminous power varies as the cube of the energy expended and unfortunately the duration or life of the lamps diminishes rapidly with the increase of light. It would, therefore, be interesting to know the relation which exists between the life of a well constructed lamp and the intensity of current.—*L'Electricien*, May 1, 1884. C.

VARIABLE BRILLIANCY OF NEPTUNE.—Maxwell Hall has made numerous observations at Jamaica, which shows that Neptune is of a bluish tint, and that its brilliancy undergoes periodical vibrations, in cycles of 7 h.

55 m. 12 s., which are probably due to rotation. He calls attention to the remarkable succession of planetary hues, corresponding to the spectral colors. Mars red, Jupiter orange, Saturn yellowish green, Uranus light green, and Neptune bluish. Prof. Pickering has made numerous photometric measurements, at the observatory of Harvard University, which show a variation of brilliancy between 7·6 and 7·9.—*L'Astronomie*, May and June, 1884. C.

Electricity in Agriculture.—M. Lestelle has devised a combination for giving warning of approaching frosts. A thermometer, placed in a battery circuit, is so arranged as to close the circuit, when the external temperature approaches the point of danger. A commutator, moved by clockwork, transmits the current of a small Ruhmkorff coil into a series of circuits. An ingenious lighter carries a match, which is kindled by the induced current, and a fuse of gun-cotton, which lights several fires almost at the same instant. These fires are provided with materials which produce clouds of smoke and ward off the frost.—*Chron. Industr.*, June 1, 1884.

Test of Glue.—The *Tischler Zeitung* gives the following method of testing glue. Carefully weigh a piece and suspend it in water, at a temperature not exceeding 10°C. (50°F.), during 24 hours. The coloring matter is then precipitated and the glue swells in consequence of the absorption of water. On removing the glue from the water the increase in weight will be found to be in proportion to the quality. The weight of the coloring matter can also be ascertained by weighing the glue a second time after it has been thoroughly dried.—*Chron. Industr.*, April 6, 1884. C.

Private Electric Lighting.—Electricity has been used in various ways for the illumination of private residences, but they are all costly. One of the best plans is that of Gaston Menier, who uses 150 Swan lamps of 40 volts and 0·7 ampère supplied by a series of 22 accumulators mounted in tension. The accumulators nominally yield from 40 to 45 ampères, which are sufficient to supply 60 lamps at a time, a number more than sufficient for ordinary occasions. The accumulators are charged each day by the aid of a continuous-current Gramme machine, excited in derivation, which is regulated by resistances introduced into the circuit. The machine is driven by a five horse-power Otto gas motor. With a little practice the servant who has charge of the lighting can accurately estimate the consumption of the evening and recharge the accumulators, allowing an excess of ten or twelve per cent. for losses and possible errors. When it is necessary to use nearly all the lamps, the direct supply from the machine is added to that of the accumulators.—*La Nature*, May 31, 1884. C.

Faye's Cosmogony.—Faye supposes that the primitive nebula had no central condensation, but that it was nearly homogeneous and spherical, not rotating, but having feeble interior gyrations in a given direction. Under the influence of internal gravity these slow gyrations formed rings, situated nearly in the same plane. These rings successively gave rise to planets, beginning with the smallest, which were nearest the centre. The rotations of the planets and the circulations of their satellites were then all

direct. During this time a central condensation was going on, becoming gradually more rapid. When the sun had absorbed all the nebula except the planets and the exterior rings, gravity, instead of varying directly as r, varied inversely as r^2. Uranus and Neptune, which were both in the form of rings, were forced to take a circulation in conformity with the new law. Neptune was entirely under this law; its rotation and that of its satellite were, therefore, markedly retrograde. Uranus was still partly influenced by the primitive nebular rotation so that its rotation is neither direct nor retrograde, but its equator is nearly perpendicular to the plain of its orbit. This hypothesis supposes the earth to have been formed before the sun so as to give geology and the natural sciences the use of all the solar heat in terrestrial organization. It also recognizes comets as parts of the solar system.—*L'Astronomie*, June, 1884. C.

[The internal gyrations are such as would result from Herschel's "subsidence" theory. The fact that earth is at the centre of the belt of greatest density was pointed out by Chase, (*Proc. Amer. Phil. Soc.*, Vols. x., seq.)]

Sand, Bricks and Stones.—M. Hignette, in the *Bulletin technologique des Ecoles nationales d'Arts et Métiers*, describes a new ceramic product from the waste sands of glass factories, which often accumulate in immense quantities so as to occasion great embarrassment. The sand is subjected to an immense hydraulic pressure, and then baked in furnaces at a high temperature, so as to produce blocks of various forms and dimensions, of a uniform white color, which are composed of almost pure silex. The crushing load is from 370 to 450 kilometres per square centimetre. The bricks, when plunged in chlorhydric and sulphuric acids, show no trace of alteration. The product has remarkable solidity and tenacity; it is not affected by the heaviest frosts or by the action of sun or rain; it resists very high temperatures, provided no flux is present; it is very light, its specific gravity being only 1·5; it is of a fine white color, which will make it sought for many architectural effects in combination with bricks or stones of other colors.—*Chron. Industr.*, May 25, 1884. C.

Intensity of Current in Geissler Tubes.—The galvanometer of Deprez d'Arsonval has been applied to the measurement of the current which is necessary for the illumination of a Geissler tube. The galvanometer was regulated to give a scale division for each hundredth-millionth of an ampère. At the moment of illumination the current was 3,500 micro-ampères; it was gradually reduced until there was a sudden extinction at 150 micro-ampères. The experiment points to great advantages which may result from the use of the galvanometer in measurements of small intensities.—*L'Electricien*, June 1, 1884. C.

Electricity and Vapor.—According to the experiments of L. J. ("Annalen der Physik," 1883, p. 518) there is no evidence of the development of electricity during the conversion of water into steam; even upon quiet electrified surfaces the steam which arises is electrically neutral. S. Kalisher has also shown that no electricity is developed by the condensation of atmospheric vapor.—*Dingler's Journal*, May 28, 1884. C.

List of Books added to the Library from January to June, 1884.

(Concluded from page 320.)

Mershon Shaking Grate. Descriptive Catalogue of. From D. Mershon's Son.

Meteorological Committee of the Royal Society. Hourly Readings for 1882. London. From the Meteorological Council.

Meteorological Committee of the Royal Society. Report of the Second Meeting held at Copenhagen, August, 1882. London. From the Society.

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[This is a work translated, compiled and published under authority of the Commissioner of Patents. Inventors and others interested in this subject will doubtless be glad to hear that at last a key has been supplied to this important publication and that by the Government of the United States. E. H.]

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DYNAMO-ELECTRIC MACHINERY.

By PROF. GEORGE FORBES, M.A., F.R.S.E.

[A Lecture delivered at the INTERNATIONAL ELECTRICAL EXHIBITION of the FRANKLIN INSTITUTE, Tuesday, September 16, 1884.]

MR. W. P. TATHAM, president of the Franklin Institute, introduced the lecturer, and spoke as follows:

LADIES AND GENTLEMEN:—It has been the earnest desire of the managers of the Franklin Institute, and particularly of Col. Banes, the chairman of the committee in charge, to give the present exhibition an educational character; and to this object they have arranged a series of lectures to be delivered in this hall on Tuesday and Thursday of each week. The present lecture will be on the subject of "Dynamo-Electric Machinery," and will be delivered by Prof. George Forbes, of London, whom I now have the honor of presenting to you.

PROFESSOR FORBES spoke as follows:

LADIES AND GENTLEMEN:—The subject of this evening's lecture is Dynamo-Electric Machinery, and it ought, perhaps, in greater strictness to be defined as dynamo-electric machinery in general, and not dynamo-electric machinery in particular; because in a single lecture it is impossible to go into the whole subject of the differences between various types of machines, and all I can hope to do in the course of this lecture is to give to you a general insight into the theory and principles of construction of dynamo machinery and of the progress

which has been made up to the present time in theoretical investigation and practical application.

At the beginning of this century our information about the action of electrical currents was extremely limited indeed, and it was not until a great discovery was made in the year 1820 that the basis was laid for those developments which have culminated in the vast number of machines which you see around you in this exhibition. At the commencement of the century, in fact up to quite a recent date, the current of electricity was developed, not by means of these machines which we see around us in the exhibition, but by means of a chemical apparatus which was called a voltaic battery, such a one as I hold in my hand here, which consists of two dissimilar metals, one of which is zinc and the other may be of carbon, copper or some other metal. When these two metals are dipped into a suitable acid and connected by a wire, a current of electricity is said to pass from the copper through that wire to the zinc. Now, if you ask me what is the current of electricity, I am bound to confess to you that I do not know. All that I can do is to give you some analogy to fix your mind upon while you are thinking about a current of electricity. If I were to take a copper wire and hold one end of it in a hot flame and hold the other end in a block of ice, heat would be absorbed in the end which was in the flame and heat would be given out in the end which was in the ice, and heat would be conducted along the wire. No material substance is conducted along that wire, and I am sure there are very few of you that can form any conception of what is passing along in the wire. But you have a notion, not a physical conception, of heat, and the phenomenon of heat transference through the wire is not unfamiliar to you. I do not say that electricity is the same as this, but it is the general opinion of scientific men that electricity passes through the wire in a somewhat similar manner to heat. It is energy and not matter which passes through the wire. That is not a tangible analogy.

I will try now to give you a clearer but grosser analogy. I allude to the analogy which exists between electricity and the flow of water in pipes. When we have a certain head or pressure of water we have also a flow of water through the pipes, and the greater the head or pressure the greater is the flow of water through the pipes. At the same time we have in the pipes a certain resistance to this flow, a resistance due to the small diameter of the pipe, and the smaller the

diameter the greater the resistance to the flow of water. In all these points electricity has an analogue. We are able to get up what we call an electric pressure or electro-motive force which exactly corresponds to the pressure of the head of water. When we have this electromotive force in the wire, we are able to get a current of electricity through the wire. That is to say, a flow of electricity which is exactly analogous to the flow of water in the pipes. But this wire offers a resistance to the passage of electricity just as the pipe offered a resistance to the flow of water, and the larger the wire is, and the greater its sectional area, the greater is the flow of electricity, just as the greater the diameter of the pipe, the greater is the flow of water. Thus, I think you will admit that we have here a tangible analogy between the flow of water and the flow of electricity; and when I speak of the electro-motive force which we have, you will understand that it is

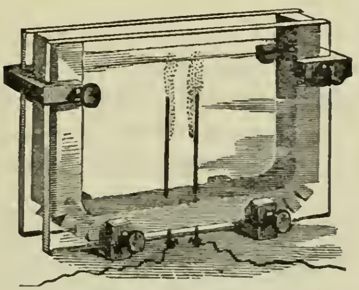

FIG. 1.—Decomposition of water by an electric current. (Cell upright.)

analogous to the pressure of water, and when I speak of the resistance it is analogous to the frictional resistance to the flow of water in the pipe.

Previous to the year 1820 the important facts which were known in connection with the electric currents were these: First, that if two wires coming from a voltaic battery were dipped into a solution of acidulated water, the elements of which the water is composed were separated or decomposed and rose as bubbles to the surface. This experiment I can show to you now by means of the magic lantern. We will lower the lights. We have now a cell which is exhibited on an enormous scale on this screen in an inverted position. (See Fig. 1.) Here is the top, and this is all air below; the cell being inverted the water which is contained in the cell is shown at the top of the picture; and here we have two wires; one is connected with the positive pole of the

voltaic battery and the other with the negative pole. If now the wires be connected we shall see bubbles of gas, hydrogen and oxygen, rising to the surface. This shows a decomposition of the water in the cell in a manner such as it would be impossible for me to show to you on the lecture table.

Another fact which was known at the same time and which was one of the few important facts known was this: that if a conductor, made thin enough, were brought in connection with the two poles of the battery it would become red hot or white hot.

I have underneath this table a voltaic battery connected with two wires coming above the table. You will be able to see as soon as I connect them with a thin piece of platinum wire that the wire becomes red hot, and if I diminish the resistance in the circuit by shortening the wire I can make it even hotter than that; I am able to make this wire of a bright red heat.

These two important facts were then known, the decomposition of water, and the heating of a conductor, by the passage of an electric current. There was another fact that was known which I ought not to omit to mention. The discovery of Sir Humphrey Davy, about the year 1811, that the spark which had often been seen on the breaking of the circuit could be intensified by using a large number of cells in a voltaic battery and taking the two ends of the wire and attaching to them two pieces of charcoal; on separating these two pieces of charcoal a brilliant light was seen to pass from one piece of the charcoal to the other. That is the fundamental experiment which led to the discovery of the electric light, that is the arc light, which is so very much used in this country.

I have in the course of the last few years visited each one of the different electrical exhibitions which have been held in Europe. Last year I was at one held in Vienna. After I had wandered about the exhibition and visited the host of applications of electricity rivalling in number those which you see in this Franklin Institute exhibition, the great variety of telegraph instruments, the hosts of different kinds of telephones and the great number of types of dynamos distributed all over the building, I happened to find myself standing in front of a small exhibit which filled me with respect and a feeling akin to awe. This was a small object placed upon velvet and covered with a glass shade, and behind it there was a bust of the illustrious man who had used this apparatus. I looked upon this instrument with respect,

because in the discovery made with it I saw the commencement and the dawn of the whole of that science of electricity which had reached such a culmination in the Vienna exhibition. I saw in that one experiment of that famous man the genesis of every one of the telegraph instruments in the exhibition, of every one of those telephones, of all the various applications of electricity, of every one of the types of dynamos in that building. I need hardly say to those acquainted with the history of electricity that I was looking at a small compass needle, and that the bust was that of the late Professor Oersted, of Copenhagen.

For many years people were aware that there was some hidden connection between the compass and electricity, between the power that impelled the compass to point to the north and the lightning in the sky. It had been believed that when lightning had disarranged the

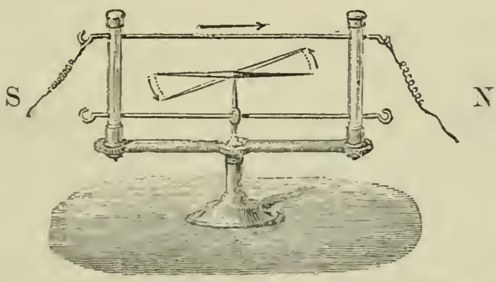

FIG. 2.—Action of an electric current on a compass needle.

compass needle and reversed its polarity it showed that there was some connection between electricity and magnetism, but no one could tell what that connection was. Mathematics were of no avail in the solution of this problem. Oersted happened to be experimenting with a battery and a compass and found the secret of the mystery, and it is from this point that we have to investigate the progress of electricity this evening.

Here we have a compass needle suspended under a wire. (See Fig. 2.) The wire will presently have an electric current passing through it. Oersted found that when the current coming from the copper pole passed over a compass needle from the south to the north, the compass needle moved with its north pole towards the west. That is, the compass needle was moved by the influence of the electric current, an action taking place between the current over it and itself. On this compass

needle I have put some pieces of paper that you may be better able to see the experiment. If Mr. Knapp, who is kindly giving me his assistance, will now lower the plates into the battery you will immediately see a declination of the north pole of the compass to the west. On breaking the current the compass needle, as you see, immediately resumes its position from north to south. In other words, when the current is flowing in this direction the north pole of the compass needle goes to your left. That is to say, if I be looking in the direction in which the current is going, the north pole of the compass needle seems to go round the wire in the direction of the hands of a watch. And if I had the compass needle at one side of the current the north pole would

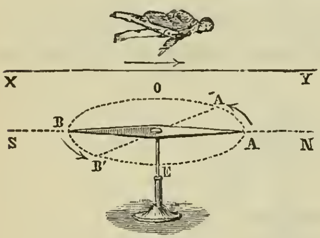

FIG. 2 *a.*

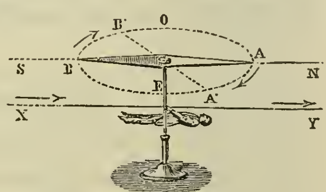

FIG. 2 *b.*

Action of an electric current on a compass needle.

still tend to turn round in the direction of the hands of a watch. The south pole tends to go in the opposite direction. Another means has been proposed for indicating the direction of motion of the compass needle. If a man be supposed to be swimming in the direction of the current and facing the compass needle, he will see the north pole going to the left. In this way, looking at Fig. 2 (*a*) and Fig. 2 (*b*), we see that a current going from south to north, if above the compass, deflects it to the west of north, and if below, to the east of north. There are various results which we can see to follow from this principle. Here I have a rough ring to illustrate the direction in which the current is going. (See Fig. 3.) Let the light arrows show the direction of the current. Now, I have found, when I am looking along these the north point appears to go round in the direction of the hands of a watch; so, also, looking at it from another point, the needle would still tend to go round in the direction of the hands of the watch, so that wherever in front of this circle I may hold the north pole of the compass needle it will always tend to go through the circle in that direction if the current is going in the direction of the arrows. Here, we have a method of

intensifying what was shown by Oersted. If I hold a compass in front of a coil the north pole will tend to be sucked in, in that direction; and the south pole would be sucked in, in a similar manner if held at the other side of the coil.

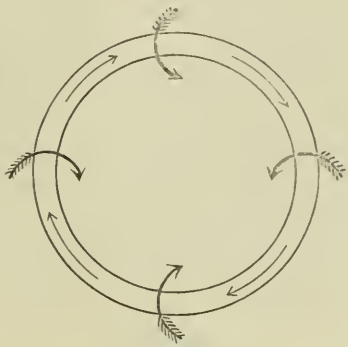

Fig. 3.—A current passing in direction of light arrows moves a north pole in direction of dark arrows.

I will show you next another experiment to illustrate the analogy between the electric current and magnetism, which follows directly from Oersted's experiments. Here I have a mass of iron or sets of bars going directly downwards with iron at their base and coils of copper wire surrounding them through which I can pass an electric current. As soon as I pass such a current through the wire this becomes a powerful magnet capable of attracting iron objects placed in its neighborhood. It will hold a bar of iron like this with enormous intensity. So, if I tumble upon the poles a quantity of nails the magnetic attraction is so great as to give the nails the appearance of cohesion which enables them to stand upright in a bridge; but as soon as I take away the current they fall down, which shows that the magnetism has left the magnet as soon as the current ceases to flow. (See Fig. 4.) In this experiment the north polarity is urged through the iron in the direction indicated by Oersted's experiment.

Having shown you how a magnet can be produced, I will show you the experiment which I described just now. Here I have a coil of wire analogous to the coil which Mr. Knapp was just now holding in his hand, and here I have a bar of iron, and I have said that if I have a north pole under here it will be sucked up if the current is going in the right direction. Now the coil will magnetize this bar. As soon

as I have magnetized the bar it has a north pole at the top, which is powerfully sucked in. The intensity of the magnetic effect of this current of electricity is so great that I have considerable difficulty in dealing with this mass of iron which I am holding in my hand.

Now, in this experiment I wish to point out to you the key that exists to the whole of the researches of Faraday which were made some years afterwards. When I have this magnetic needle, which is deflected by the current, or this bar which is sucked into the coil, it is evident to every person of sense here that work is being done. That is to say, that when the needle is moved away from the position which it naturally occupies, work is being done. Where does this work come from? It comes from the interaction of the compass needle and the electric current. It is, therefore, safe to say that when a compass needle is moved in the neighborhood of an electric current it is doing

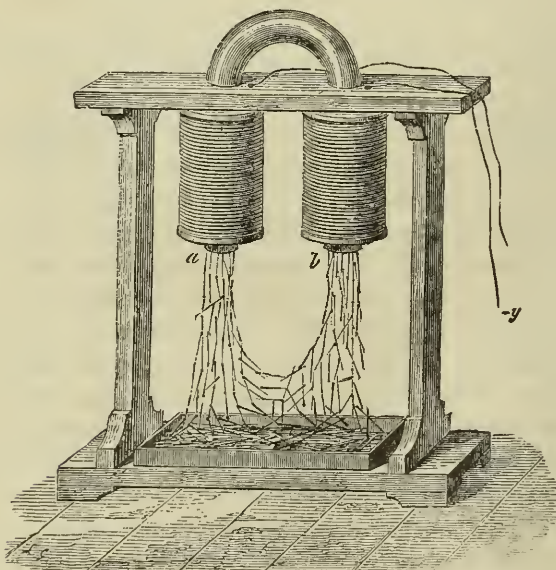

FIG. 4.—Electro-magnet supporting a bridge of nails.

work, and in order to do work it must take away a part of the current from that current which is acting. Now, in order to take away from the current it must have started a feeble opposing current in an opposite direction. And here is the point that I wish you to grasp, that in the movement of the compass needle from one side or the other, an

opposing electro-motive force has been created, that is, a force tending to set up a current in the wire in the opposite direction. This I believe was the manner in which Faraday argued the matter out in his mind and that is how he was able to originate the experiments which he made to produce the opposite result, that is, to create a current in the wire.

Hitherto, Oersted had shown that a current could produce motion in a magnet; Faraday conceived the idea that by the motion of the magnet we could produce a current in the wire. This experiment I shall now be able to show you. By moving a magnet pole into a coil you will be able to see a movement of the pointer which I have here and which indicates the strength of currents which are made to flow

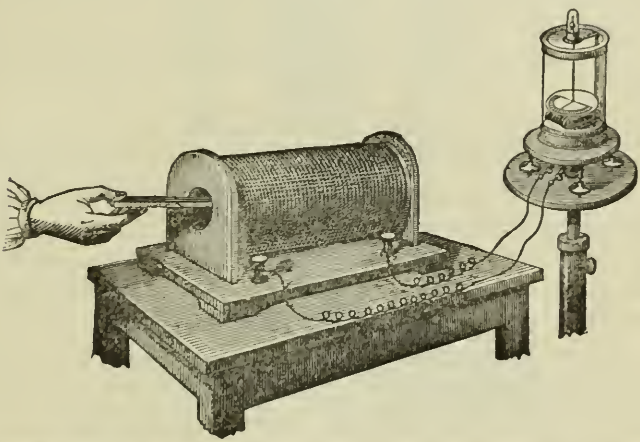

FIG. 5.—Galvanometer showing an induced current while a magnet is moving into a coil of wire.

through wires which are connected with it. This instrument which I have here is a galvanometer, an instrument whose object is to measure the strength of electric currents. You see the index traveling over the scale at the present moment; it now points almost to the middle of the scale. If then, I take an ordinary coil of wire and reverse the experiment which I was talking of a short time ago, that is to say, pass the pole of the magnet into the coil of wire, we shall find that the electro-motive force is started in the coil of wire, and if the circuit of the wire forming the coil be completed through the galvanometer, we shall find that a current is created which will show itself by the deflection of the galvanometer. (See Fig. 5.) Now, in order that this be made distinct,

I shall ask Mr. Knapp suddenly to insert the north pole of the magnet into the coil of wire. The movement of the galvanometer will be very slight, but you will be able to see it. I will now ask him to withdraw the pole and the movement will be in the opposite direction. In this manner I get up a sort of swing in the galvanometer which is more visible to you and shows that I can move the galvanometer needle from one side to the other when I put in the north pole of the compass or draw it out. The motion you see is very slight, but I am perfectly able to control it; it moves always to the right when I put the pole in and always to the left when I take the pole out. This movement of the index proves that Faraday's notion was right.

I am anxious now to show you this experiment in another way, for it shows a further development of our notion which will enable you to understand the application of Faraday's principle to the construction of the dynamo machine, which we will be investigating in a few minutes.

Here I have a coil of wire connected up with the galvanometer. In the coil there is a bar of iron. I show you that when I magnetize the bar of iron which goes through it, there will be a deflection of the galvanometer, and when I reverse the magnetism, there will be a deflection in the opposite direction; so that when I have a piece of iron enclosed in the coil of wire all I have to do is to magnetize it by the influence of these neighboring poles. Mr. Knapp will now magnetize the pole by touching the end with a magnet; as soon as it is magnetized there is a movement to the right. Reverse now—there is a movement to the left. This is merely an experiment which assists our comprehension of the construction of the machine, or shows that magnetizing the iron core produces the same effect as the introduction of the magnetic pole into the system.

Now let us see how all these principles, discovered by Faraday, can be applied to the construction of dynamo-electrical machines. You have seen when I have a coil here with the north pole introduced through it that there is then a current of electricity established in it, and that current is established only while the north pole is passing through it. When the magnet is fixed no action is taking place. I have introduced the north pole into the coil and if I withdraw that north pole by the way it went in, I shall of course create a current in the opposite direction. If, on the other hand, I carry the south pole in the same direction in which the north pole has gone, it also will create

a current in the opposite direction. After I have passed the north pole through the coil, it is impossible to increase the current in that direction. The further motion of the other pole must produce a current in the opposite direction to the current first produced. However I may act in this way, I am bound to get a motion which gives alternate currents. If I use a bar of iron surrounded by a coil of wire and magnetize it, with the north at one end and the south at the other end

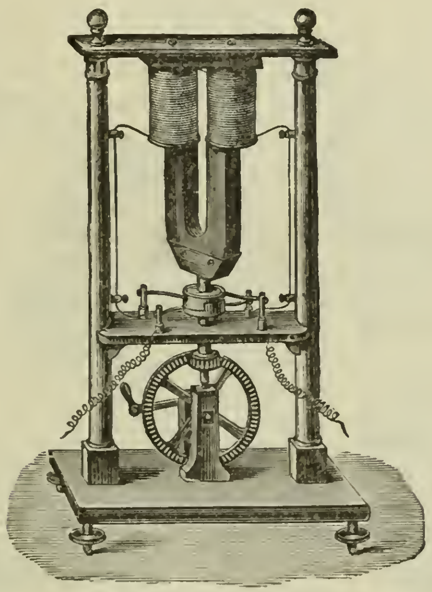

FIG. 6.—Pixii's machine.

I get a current, but it is impossible for me to keep that current going because I cannot get beyond a certain amount of magnetization. If I reverse the magnetization I produce a current in the opposite direction at the moment of reversal.

It would seem then that we have the power of producing currents alternating in direction. This was the only type of machine known for a considerable time; and all of you have seen those instruments in which a handle was turned rapidly while a person held the ends of the wire in his hand and received a succession of severe shocks; that was due to the production of alternate currents by an arrangement which alternately magnetized and demagnetized the iron core inside the coil.

I will now show you some illustrations on the screen which show

the construction of machines for producing alternate currents in this manner.

This in one of the alternate current machines, like that of Pixii or Saxton (Fig. 6). Here we have a horse-shoe magnet fixed about an axis to which a rapid rotation can be given. At the top are two coils which remain fixed, and inside these coils there are bars of iron. The consequence of this is that when the magnet is rapidly rotated the

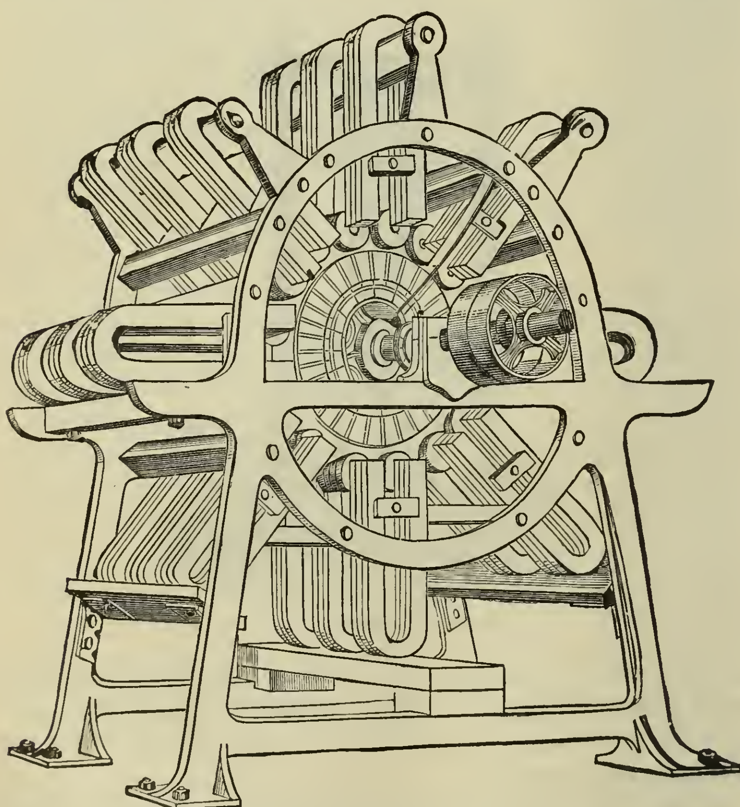

FIG. 7.—The Alliance machine for alternate currents.

north pole and the south pole will be successively presented to the coils, and the result is a continual reversal of the magnetism in those coils; and every time the magnetism is reversed a current is created in one direction or the other; and if the wires in these coils are held in the hand, a succession of violent shocks will be felt by the person hold-

ing them. One of the original machines of this type is shown in the historical collection in this exhibition.

I will put into the lantern another slide, showing a different form of machine. Here again is a horse shoe magnet. The magnet does not revolve, but the coils of wire revolve in front of the poles of the magnet. Here are the two coils at the bottom mounted upon a spindle which is horizontal. These two coils are rotated rapidly. Inside the coils are cores of iron. The cores of iron are magnetized in opposite directions as they pass from one pole of the fixed magnet to the other pole, and every time they do so, the current is changed in direction. Here also we are able to receive shocks from the coils by holding the ends of the wire in the hand. In this machine, as constructed, there was an arrangement for changing the direction of the current in the outer circuit at the same time that the magnetism was changed, but of that I will speak later.

In the next slide I shall show you the most practical of all the older machines (Fig. 7). This was the Alliance Machine used for lighthouses in Great Britain and France. It consists of a vast number of horse-shoe magnets. This machine has actually been employed in in our lighthouses in France and England. At the present moment you may see a machine made by De Meritens which is exhibited by James W. Queen & Co., at the far end of this Exhibition and which is constructed on the same principle.

A large number of rows of horse-shoe magnets are about this central axis, around which also there is a ring of coils of wire each with iron enclosed, and all these coils pass in succession the poles of the magnets and so alternate currents are created in the coils which produce a current of great intensity. These powerful currents are generated by means of a steam engine and so we were able to get the powerful spark of the arc lamp like what was seen by Sir Humphrey Davy, in 1811.

I may now pass on rapidly to the greatest improvement which was introduced, and that was the continuous current machine. It was found possible to take these currents from the machine which were alternately in opposite direction and reverse their direction through the circuit, that is to say through an arc lamp or through the hands of the person holding the wires, and thus a continuous current always in the same direction was made. A single reversal, however, did not produce a steady current and it was a long time afterwards before means were devised for creating a steady current which was of real use in produc-

ing these continuous currents which we now use in electric lighting, and in this country in telegraphy.

The greatest improvement was introduced by Mr. Gramme about the year 1870, and he did this by means of a machine, a diagram of which you can see on the board here. This is only a rough diagram showing the principle of the machine. (Fig. 8.)

Here is the north pole of the field-magnet, marked "*N*," and here is the south pole, marked "*S*." These are magnetized by coils going around the mass of iron. Here is a ring of iron with coils of wire wound around it. This is the armature. The iron of the ring becomes magnetized by the influence of these north and south poles of the field

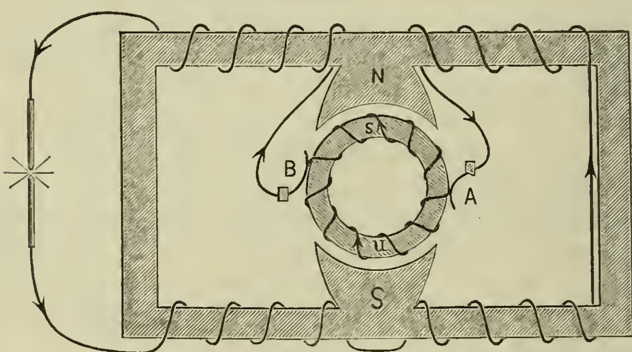

FIG. 8.—Diagram of Gramme machine (series wound), showing field-magnet and pole pieces, ring armature, commutator, and direction of currents. The armature rotates with the hands of a watch.

magnet and the poles in the ring retain the same position in space while the ring revolves. As soon as the ring rotates it follows that these wire coils are continually passing over the pole, or I might say this pole in the iron is continually passing through the coils. In other words, an electric current is being continually developed through successive coils as they pass through here.

If you look at the sketch you will see that there is a south pole in the ring at *s* opposite to the north pole in the field magnet, and a north pole in the ring at *n* opposite to the south pole of the field magnet. If you remember the rule I have given you about the direction of currents you will be able to trace their direction in the machine. Suppose the ring to be revolving in the direction of the hands of a watch, the north pole will be going in the opposite direc-

tion through the coils, and if the eye be placed so that the north pole is coming towards it the induced current circulates with the hands of a watch; with a south pole the reverse takes place. Follow this out and you will see that the current flows in the upper semicircle from *A* through *s* to *B*, and in the lower one from *A* through *n* to *B*. But the wire is continuous, and if metallic brushes take up the current at *A* and *B* you see that the current will go out of the armature coils at *B*, through the circuit, and in again at *A*.

Mr. Gramme constructed a commutator with a large number of bars to which the ends of these coils were connected, and he used metal brushes to rub on them, and thus there was a continual series of changes going on as each coil passed those brushes, and thus he was enabled to get a very continuous current of electricity through his machine.

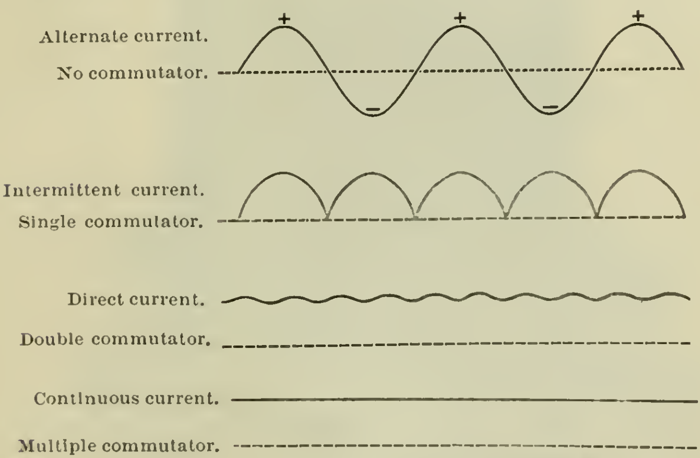

FIG. 9.—Action of commutators.

I have put up here a small diagram to illustrate the action of these commutators. (Fig. 9.) In the old alternate current machine there is a bobbin passing the poles of the magnet; the current increases to a maximum and falls to a minimum then increases to a maximum in the opposite direction when there is no commutator employed. When we put in a single commutator so as to reverse the current in each pole it rises to a maximum and falls to a minimum and we get this interrupted current, but always in the same direction. That was the first kind of continuous current made. Some machines were introduced to

give a double commutation that is twice commutated in the course of a revolution, and thus an irregular sort of current was produced; but when Gramme introduced his machine with a vast number of commutations in one revolution he had a vast number of currents and thus there is an almost continuous current produced by the Gramme machine.

In the Gramme machine which I showed you here, the current

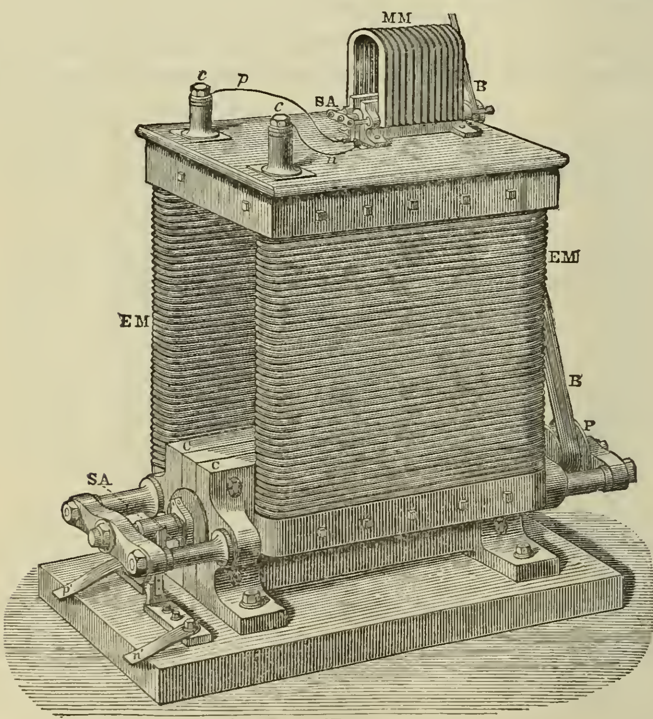

FIG. 10.—Wilde's magneto-electric machine.

itself is used to excite the magnetism in what are called the field magnets, and it passes through this field magnet before going to the lamp circuit. That is what is called winding the field magnets in series. Sometimes the current is taken direct from the revolving portion to the lamp circuit and a second current is taken to the field magnets forming two circuits and thus the field magnets are said to be magnetized in a shunt. The practice of using the current which is induced to

produce the magnetism of the field magnet was a completely new thing in 1866, and was invented simultaneously by several people, notably by Wheatstone and Siemens, who produced papers describing it on the same day in the Royal Society of London.

I will now show you on the screen a few more machines of the continuous current nature, including a Gramme machine, and some of the details of the Gramme machine, and I hope when you have seen these diagrams you will be able when going around this exhibition to study the nature of the different machines. It is in the arrangement of the field magnet and armatures in which the machines vary. The whole evening might be given to the variations of the different machines;

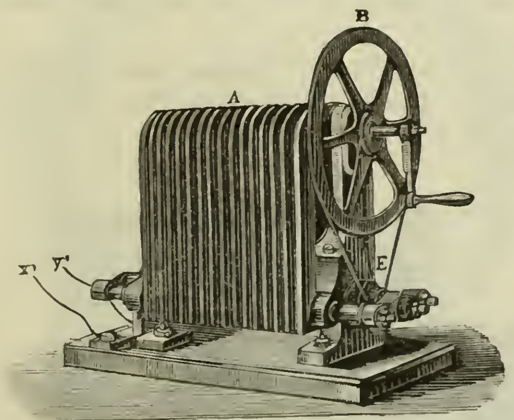

FIG. 11.—Siemens' machine.

in the meantime I can only show you a very few illustrations of the different types of machines.

The first machine which I have to show you here is the machine of Wilde (Fig. 10). Allow me to call your attention to the special parts. *S A* is the axis about which it revolves. In this revolving armature electricity is induced. This is the seat of the electro-motive force; this is the pump which pumps the water and gives it its pressure, and enables it to go through the circuit. *E M* are the magnets, which are composed of soft iron, magnetized by the coils of wire surrounding them. In Wilde's machine, he had a small machine, *M M*, with permanent steel magnets, fixed on top of the larger one. They are not so powerful magnets as these electro-magnets, and he used them to induce a small current of electricity instead of using the larger current to mag-

netize these electro-magnets. It created a feeble current, which circulated around these field magnets, and gave an intense magnetism to these pole pieces *C C.* These pole pieces being polarized, and the armature revolving, the coils of the armature developed an electric current of

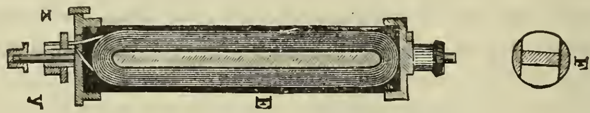

FIG. 12.—Armature of Siemens' machine.

great intensity, far surpassing the current developed by the little machine. Thus he was enabled to get a very great current, sufficient for electric lighting.

This next slide is a Siemens' machine, made also with permanent horse shoe magnets (Fig. 11). You will see the revolving part attached to the driving gear at *E.* That revolving part is of course the armature;

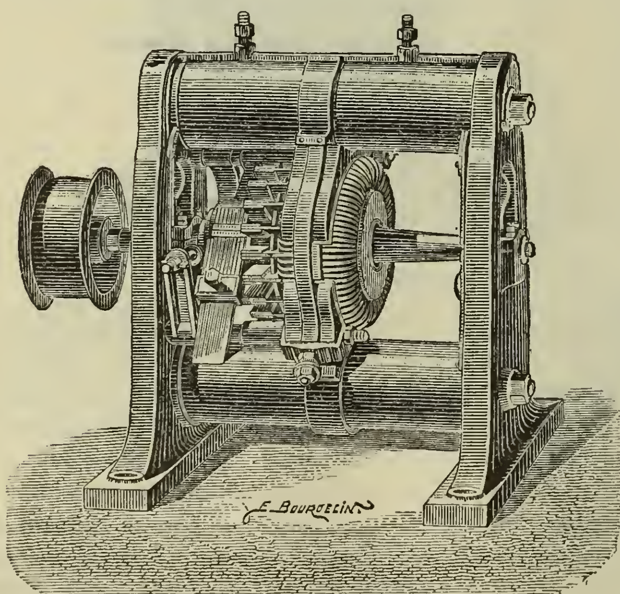

FIG. 13.—A Gramme machine.

the coils of the armature being between the pole pieces. This central part is a mass of iron, and the coils of wire run longitudinally around the core. (Fig. 12.) This machine, as originally constructed, had simply

a single commutator, which reversed the current once in each revolution, which sent a continuous but varying current through the circuit.

This slide (Fig. 13) is a Gramme machine, and was introduced to the public shortly after 1870, and this machine has been the basis of nearly all the dynamo machines seen in the world. These round parts at the bottom and top, surrounded by coils of wire, and the pole pieces seen at the top and bottom, are the magnets. There is the Gramme

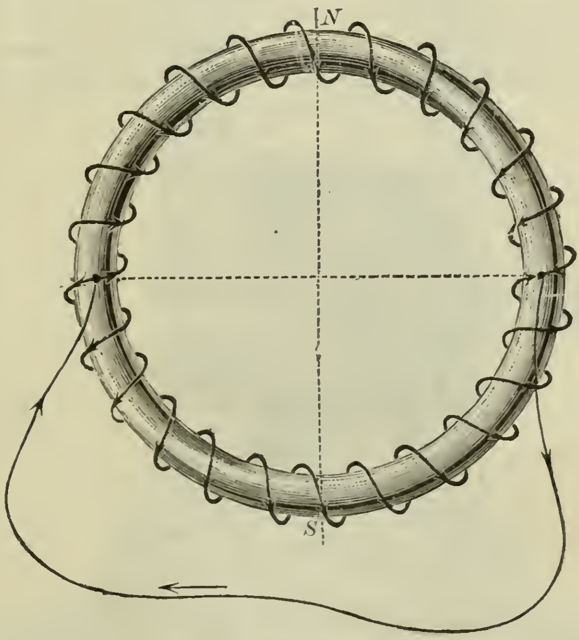

FIG. 14.—Ideal Gramme ring.

armature, which rotates about the horizontal axis, and here is the commutator.

This next picture is an ideal Gramme ring (Fig. 14). Here are are the successive coils. Here is the south and here is the north pole of the ring. You will notice that in each coil, as it passes the pole, an electric current is developed. The current will always be in the same direction, at any part of the ring, and very nearly continuous.

The next picture is a portion of a Gramme ring (Fig. 15). It shows the way it is made up; it consists of a central core of iron wire and coils of insulated wire successively laid upon it. It is cut, showing the ends of the wires projecting.

Here is a small Gramme machine, which can be worked by hand (Fig. 16). In construction it is exactly similar. Here are the poles; there is the revolving ring; the commutator is on the other side, and cannot be seen.

This picture is of a machine of very remarkable historical interest (Fig. 17). This was designed by Pacinotti, and described about the year 1864. It is exactly like a Gramme ring in many points. It revolves around a vertical axis instead of a horizontal axis, and as it revolves the current is picked up by a commutator. This was made on a small scale, and was not used for commercial purposes. It is conclusively proved, however, that Gramme did not know of the work

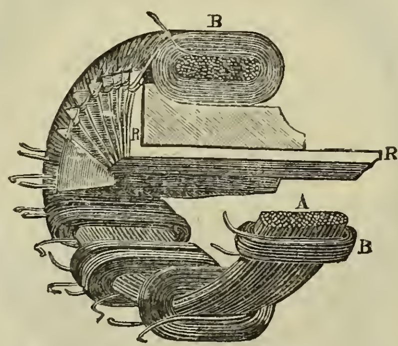

FIG. 15.—Portion of a Gramme ring, showing the mode of construction. A is a section of the iron ring made up of iron wire. B is a coil with the wire ends ready for attachment to commutator bars.

of Pacinotti, but it is incontestible that Pacinotti was the inventor of the type from which all the other machines have been constructed.

An attempt has been made to improve the constancy of the current, and prevent the difference in intensity which is observed when we are using a large or small number of lights. It is found with some kinds of machines that the intensity of the electro-motive force increases when we add lamps to the circuit, and in other kinds we find the opposite is the case. When the field-magnet of a machine is wound as a shunt, as we diminish the number of lamps, the lamps left burning increase in brightness. The machine which illuminates this hall is one with a shunt. It is the large Edison machine. This machine will not show the effect very much, because the armature of that machine is made of

very low resistance, and it is only lighting 300 lamps instead of 1,500; and the electro-motive force or pressure is steady, however we vary the number of lamps. Still it is a shunt-wound machine, and there is a slight difference between the intensity of the lights.

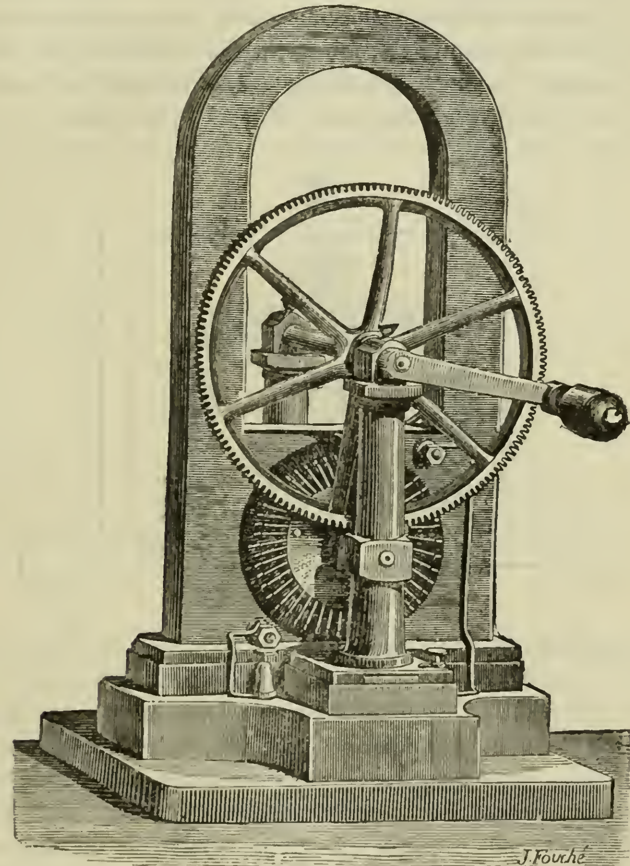

FIG. 16.—Hand Gramme machine.

I will now put out the lights in succession, and leave these two lamps to be last put out. You will notice that there is a slight increase in the brightness of the last two lights; it remains with you to notice that the change is very slight indeed. This illustrates how we can make a machine that will regulate itself; that is, however many lamps

we put upon the circuit the electrical pressure is the same and the intensity remains constant.

Now watch these two lights as the others are put out in succession. You see they are decidedly getting brighter. Put them in again and you will see the opposite effect take place. You will see them dimming. Now, this is distinctly dimmer than it was a little ago, though but very little. It is quite evident to you that a machine like the Edison (the machine producing these lights is what is called the Jumbo) does not vary much when we vary the number of lamps up to 300.

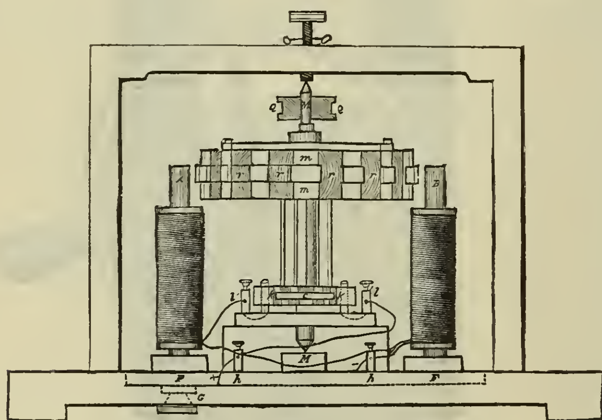

FIG. 17.—Pacinotti's machine.

It is astonishing when we descend to one lamp and show such a small variation in the light. In an ordinary small machine the change of brightness would have been decided. In the shunt wound machine, when we increase the number of lights we diminish the intensity of each. In the series machine the opposite is the fact. I might show you that experiment, but time is passing quickly. That is to say, when we put in one lamp in a series machine it hardly glows at all. The reason is that the current is very feeble with one lamp, because the resistance in the circuit is very great. When the current is feeble the magnetism of the field magnets is feeble, and that is the cause of the low electro-motive force. As I increase the number of lamps I am increasing the means of escape; that is, I am increasing the number of pipes of my hydraulic system, and letting more water flow, and the magnetization is more complete.

Of late years it has been earnestly attempted to produce perfect

equality even in the small machines, and without going to the vast dimensions of the large-size Edison machines, by winding the field magnet in a double way, partly as series machine and partly as shunt machine. I have already explained to you that when we have a shunt-wound machine the electro-motive force gradually diminishes as we put in more lamps; and I have shown that in the series machine the electro-motive force gradually increases as we reduce the resistance and put in more lamps. Therefore, taking the mean of these, or summing these two curves together (Fig. 18), by winding these two magnets partly in series and partly in shunt, the electro-motive force does not

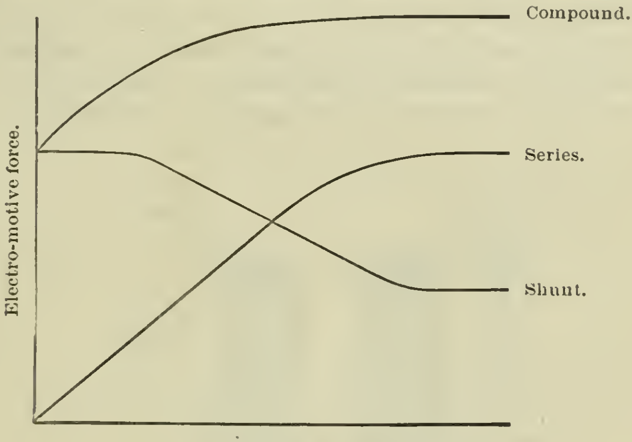

FIG. 18.—Showing relative advantage of compound winding in maintaining the electro-motive force constant when the current is changed.

vary in so great a degree. Thus we are able to get by a proper adjustment a perfect equality or difference of potential between the two parts of the line, and, whether we are dealing with the terminals, at the machine itself, or where the lamps are applied a mile away, by a proper compound winding, partly in series and partly in shunt, we are able to produce a compound machine which gives a constant electro-motive force however many lamps we are using, that is, however many pipes we may have drawing off water from our supply.

I had hoped to have spoken about the effects of alternate current machines, which would have been extremely interesting; but time is passing so rapidly that it is impossible to deal with this subject now.

I will only show you one remarkable experiment by means of the alternate current passed through a coil of wire on one arm of the electro magnet and through an incandescent lamp.

I will show these effects, because they are very remarkable. The effects of the alternate current machines are very difficult to understand at first sight. Perhaps, if I explain to you this one action I may have given some information to some of my audience here, and it may enable them to see some of the difficulties to be met with in alternate current machines, and enable them to avoid making mistakes in their applications in the future. I will pass an alternate current through one coil of the electro-magnet, and also through one incandescent lamp. That lamp glows very feebly indeed. But as soon as I join the terminals of this wire, going round the second coil of the electro-magnet, it immediately glows brightly. (See Fig. 19.)

When I have an alternate current passing through this first core, it

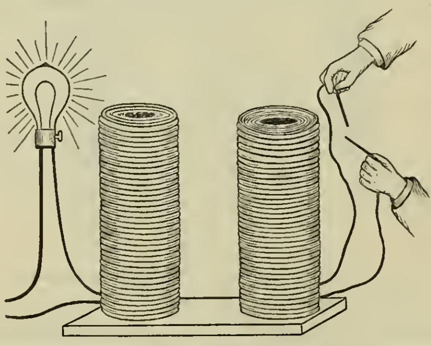

FIG. 19.—An alternate current passing through one arm of an electro-magnet and an incandescent lamp. When the ends of the other coil are separated the lamp is dull; when joined the lamp is bright.

is exercising a large power in magnetizing and demagnetizing that core. That core exercises its influence upon the other core to magnetize and demagnetize it. Thus the iron in these two arms of the magnet is being magnetized and demagnetized with great rapidity, and it is taking away from the current which is feeding this lamp. But so soon as I connect the wires of this other coil I am using up the power and magnetism in this core. I am making it do work in creating currents in this second coil ; that is to say, this second coil acts as a drag on the magnetism of the core. So soon, then, as I connect the

ends of the wire of this second coil the magnetism cannot change there so suddenly, and the primary current is not able to do so much work in the magnet, and therefore it is more free to exert its own force, and therefore it enables the lamp to glow with greater brightness than it did before.

I must not trespass upon your time longer; I feel that I have already exhausted the limits which I had set myself, and which ought to be set to a lecture of this kind. On the subject of dynamo-electric machines several lectures might advantageously be given. We have, I think, advanced a great distance in the way of theoretical application of the laws of electricity to the dynamo-electric machines; we owe a great deal of this not only to the advance in theory, but also to the advance in the application of theory. There is a great deal still to be done by theory and by practical application. If we look to the past we shall find that there has been too much hesitation on the part of practical men in accepting the results of theory; and if we look with hope to the future we shall see theoretical views put to practice to guide us and direct us in our efforts to arrive at perfection. In fact I may say that I have seen private experiments which lead me to predict remarkable progress in dynamo-electric machinery in the near future.

On a recent occasion Lord Rayleigh, the president of the British Association, at Montreal, and also the very distinguished professor at the University of Cambridge, England, stated that he was astonished to find, considering how well known were the laws of induction in the time of Faraday, and how complete was our knowledge of electrical induction in those years long past, he was astonished to find at how slow a rate the practical applications of those principles long ago published by Faraday had been introduced into every-day work. Lord Rayleigh then stated his opinion that the cause of this slowness to apply the achievements of science was due to want of faith, and I agree with him that this is the true solution of the enigma. Faraday was a man whose mind was taken up with original investigation. Faraday had as complete a mathematical conception of the theory of electricity as any of us who have studied Clerk Maxwell's book could have; although he could not express one word of his ideas in terms of x, y and z. And in this way from the experiment of Oersted he was able to divine in his own mind the consequences of that experiment, and foresee the results of his own experiments in developing electro-magnetic induction; whereas you would have thought it was only by the

application of the higher mathematics that this could have been done. He also had this peculiarity, that he knew what his bent was, and in what manner he could be of the most benefit to mankind. He says himself, in one of his writings, as much as this. In speaking of the possible applications, and throwing out a hint as to the manner in which powerful dynamo-electric currents might be created, he makes use of the following language: "But in all my work it has been my endeavor not to seek out applications in practice so much as to arrive at new principles." I speak from memory, but I think these are nearly the words that appear in his "Researches." This was the key to his life. He knew that he had an inherent power to discover new principles, and that if he left the applications to others they would come in time.

In this past history of the applications of electricity we have thus to remember that thirty, forty and even fifty years ago we had the knowledge which was given us by Faraday, which would have enabled us to construct this dynamo-electric machine, and it was only through want of faith in those whose duty it is to deal with the applications of scientific facts that the slowness has been due. And in future let it be hoped that theory and application will work hand in hand, and that, while theorists see that the value of their investigations and the impetus which prompts them are due to their practical application, so others whose duty it is to apply science commercially may see that the rapidity of these applications depends upon the acceptance of the results of theory; and if this exhibition and this course of lectures lead those who apply science to study a little more fully the new ideas which theory is teaching us, and if they lead theorists to see a wider range for the applications of theory, then the managers of this Institute will have had the best reward possible for giving these lectures in knowing that they have been of some avail.

MR. JOSEPH M. WILSON:—We are all so very much indebted to Prof. Forbes for his exceedingly interesting lecture, that I feel I am only expressing the sentiments of those present when I move that a vote of thanks be extended him, to be indicated by the audience rising.

The motion was seconded by Dr. William H. Wahl, and was unanimously adopted.

NOTE.—A number of the illustrations in Prof. Forbes' lecture are from "*Schellen (Keith), Magneto and Dynamo-Electric Machines*, for the use of which the JOURNAL is indebted to the politeness of the publisher, D. Van Nostrand, New York.

STEAM BOILERS AS MAGAZINES OF EXPLOSIVE ENERGY.

By Professor Robert H. Thurston.

[A paper read before the American Society of Mechanical Engineers, October, 1884.]

Section I.—Computation of Stored Energy.

In the following paper it is proposed to present the results of a series of calculations relating to the magnitude of the store of energy contained in masses of steam and of water, when heated to temperatures customarily met with in the various applications of the expansive power of steam, in the arts, and especially in steam boilers. This energy may be measured by the amount of work which may be obtained by the gradual reduction of the temperature of the mass to that due atmospheric pressure by continuous expansion.

The subject is one which has often attracted the attention of both the man of science and the engineer. Its importance, both from the standpoint of pure science and from that of science applied in engineering and the minor arts, is such as would justify the expenditure of vastly more time and attention than has ever yet been given it. The first attempt to calculate the amount of energy latent in steam boilers, and capable of greater or less utilization in expansion by explosion, was made by Mr. George Biddle Airy,* the Astronomer Royal of Great Britain, in the year 1863, and by the late Professor Rankine† at about the same time. Mr. Airy and Professor Rankine published papers on this subject in the same number of the *Philosophical Magazine* (Nov., 1863), the one dated the 3d of September and the other the 5th October of that year. The former had already presented an abstract of his work at the meeting of the British Association of that year.

In the first of these papers, it is remarked that "very little of the destructive effect of an explosion is due to the steam which is confined in the steam-chamber at the moment of the explosion. The rupture of the boiler is due to the expansive power common at the moment to the steam and the water, both at a temperature higher than the boiling point; but as soon as the steam escapes, and thereby diminishes the

* "Numerical Expression of the Destructive Energy in the Explosions of Steam Boilers."

† "On the Expansive Energy of Heated Water."

compressive force upon the water, a new issue of steam takes place from the water, reducing its temperature; when this escapes, and further diminishes the compressive force, another issue of steam of lower elastic force from the water takes place, again reducing its temperature; and so on, till at length the temperature of the water is reduced to the atmospheric boiling point, and the pressure of the steam (or rather the excess of steam-pressure over atmospheric pressure) is reduced to 0."

Thus it is shown that it is the enormous quantity of steam so produced from the water, during this continuous but exceedingly rapid operation, that produces the destructive effect of steam-boiler explosions. The action of the steam which may happen to be present in the steam-space at the instant of rupture is considered unimportant.

Mr. Airy had, as early as 1849, endeavored to determine the magnitude of the effect thus capable of being produced, but had been unable to do so in consequence of deficiency of data. His determinations, as published finally, were made at his request by Professor W. H. Miller. The data used are the results of the experiments of Regnault and of Fairbairn and Tate, on the relations of pressure, volume and temperature of steam, and of an experiment by Mr. George Biddle, by which it was found that a locomotive boiler, at four atmospheres pressure, discharged one-eighth of its liquid contents by the process of continuous vaporization above outlined, when, the fire being removed, the pressure was reduced to that of the atmosphere. The process of calculation assumes the steam so formed to be applied to do work expanding down to the boiling point, in the operation. The work so done is compared with that of exploding gunpowder, and the conclusion finally reached is that "the destructive energy of one cubic foot of water, at a temperature which produces the pressure of 60 lbs. to the square inch, is equal to that of one pound of gunpowder."

The work of Rankine is more exact and more complete, as well as of greater practical utility. The method adopted is that to be described presently, and involves the application of the formulas for the transformation of heat into work which had been ten years earlier derived by Rankine and by Clausius, independently. This paper would seem to have been brought out by the suggestion made by Airy at the meeting of the British Association. Rankine shows that the energy developed during this, which is an adiabatic method of expansion, depends solely upon the specific heat and the temperatures at the beginning and the end of the expansion, and has no dependence, in any manner, upon

any other physical properties of the liquid. He then shows how the quantity of energy latent in heated water may be calculated, and gives, in illustration, the amount so determined for eight temperatures exceeding the boiling point. Approximate empirical expressions are given for the calculation of the energy and of the ultimate volumes assumed during expansion as follows, in British and in Metric measures:

$$U = \frac{772\,(T - 212)^2}{T + 1134{\cdot}4}; \qquad U_m = \frac{423{\cdot}55\,(T - 100)^2}{T + 648};$$

$$V = \frac{36{\cdot}76\,(T - 212)}{T + 1134{\cdot}4}; \qquad V_m = \frac{2{\cdot}29\,(T - 100)}{T + 648}.$$

These formulas give the energy in foot-pounds and kilogrammeters, and the volumes in cubic feet and cubic meters, T being taken on the common scale. They may be used for temperatures not found in the tables to be given, but, in view of the completeness of the latter, it will probably be seldom necessary for the engineer to resort to them.

This subject attracted the attention of the writer at a very early date. Familiarity, from early boyhood, with the destructive effects of steam boiler explosions, the singular mystery that has been supposed to surround their causes, the frequent calls made upon him, in the course of his professional practice and of his studies, to examine the subject and to give advice in matters relating to the use of steam, and many other hardly less controlling circumstances, invested this matter with an extraordinary interest. Probably no subject, within the whole range of the practice of the engineer has demanded or has received more attention than this; and probably no such subject is to-day less satisfactorily developed in theory and less thoroughly investigated experimentally than this. It is one which the writer has endeavored, at several different periods in the course of his work, to take up and reduce, if possible, to a consistent theoretical and practically applicable form. On each occasion, however, his labors were interrupted before they were fairly begun.

In the year 1872 the writer received from the Secretary of the Treasury of the United States a communication in which he was requested to prepare, for the use of the Treasury department, a report on the causes and the conditions leading to the explosions of steam boilers, and he began the preparation of such a report, in which he proposed to incorporate the facts to be here presented. In the year

1875, the writer, then a member of a commission formed by the government to investigate the subject, was asked by the Cabinet officer having direction of the matter to accept the chairmanship of the commission and to give his time to the subject under investigation. For sufficient reasons he was unwilling to undertake the work, and, an older and wiser head was appointed, at his request. A little later, ill health compelled him to resign from the commission; but his brief connection with the board led them to the further study of the subject of this paper; the investigation was, however, again interrupted, and has not since been taken up in the systematic manner then proposed.

In this paper, it is proposed to limit the subject to the investigation of the quantity of energy stored in some of the familiar and commonly used forms of steam boilers which are now everywhere seen endangering, to a greater or less extent, the lives and property of all who may be either permanently or temporarily within range of them.

A steam boiler is a vessel in which is confined a mass of water, and of steam, at a high temperature, and at a pressure greatly in excess of that of the surrounding atmosphere. The sudden expansion of this mass from its initial pressure down to that of the external air, occurring against the resistance of its "shell" or other masses of matter, may develop a very great amount of work by the transformation of its heat into mechanical energy, and may cause, as daily occurring accidents remind us, an enormous destruction of life and property. The enclosed fluid consists, in most cases, of a small weight of steam and a great weight of water. In a boiler of a once common and still not uncommon marine type, the writer found the weight of steam to be less than 250 pounds (114 kilogs.) while the weight of water was nearly 40,000 pounds (18,144 kilogs.). As will be seen later, under such conditions, the quantity of energy stored in the water is vastly in excess of that contained in the steam, notwithstanding the fact that the amount of energy per unit of weight of fluid is enormously the greater in the steam. A pound of steam, at a pressure of six atmospheres (88·2 pounds per square inch), above zero of pressure, and at its normal temperature, 177C. (319°F.), has stored in it about 125 British Thermal Units (32 Calories), or nearly 100,000 foot-pounds of mechanical energy (13,825 kilog. meters) per unit of weight, in excess of that which it contains after expansion to atmospheric pressure. A pound of water accompanying that steam, and at the same pressure, has stored within it but about one-sixteenth as much available energy. Never-

theless, the disproportion of weight of two fluids is so much greater as to make the quantity of energy stored in the steam contained in the boiler quite insignificant in comparison with that contained in the water. These facts will be fully illustrated by the figures to be hereafter presented.

The quantity of work and of energy which may be liberated by the explosion, or utilized by the expansion, of a mass of mingled steam and water has been shown by Rankine and by Clausius, who determined this quantity almost simultaneously, to be easily expressed in terms of the two temperatures between which the expansion takes place.

When a mass of steam, originally dry, but saturated, expands from an initial absolute temperature, T_1, to a final absolute temperature, T_2, if J is the mechanical equivalent of the unit of the heat and H is the measure, in the same units, of the latent heat per unit of weight of steam, the total quantity of energy exerted by unity of weight of the expanding mass is, as a maximum,

$$U = JT_2 \left(\frac{T_1}{T_2} - 1 - \text{hyp. log. } \frac{T_1}{T_2} \right) + \frac{T_1 - T_2}{T_1} H. \qquad \text{(A).}$$

This equation was published by Rankine a generation ago.*

When a mingled mass of steam and water similarly expands, if M represents the weight of the total mass and m is the weight of steam alone, the work done by expansion will be measured by the expression,

$$U = M\,J\,T_2 \left(\frac{T_1}{T_2} - 1 - \text{hyp. log. } \frac{T_1}{T_2} \right) + m\,\frac{T_1 - T_2}{T_2}\,H \qquad \text{(B).}$$

This equation was published by Clausius in substantially this form.†

It is evident that the latent heat of the quantity m, which is represented by $m\,H$, becomes zero when the mass consists solely of water, and that the first term of the second member of the equation measures the amount of energy of heated water which may be set free, or converted into mechanical energy by explosion. The available energy of heated water, when explosion occurs is thus easily measurable.

As has already been stated, this method was first applied by Rankine to the determination of the available energy of heated water for several selected temperatures and pressures. It has long been the intention of the writer to ascertain the magnitude of the quantities of energy

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* Steam Engine and Prime Movers, p. 387.

† Mechanical Theory of Heat, Browne's Translation, p. 283.

residing, in available form, in both steam and water, for the whole usual range of temperatures and pressures familiar to the engineer, and also to carry out the calculations for temperatures and pressures not yet attained, except experimentally, but which are likely to be reached in the course of time, as the constantly progressing increase now observable goes on. The maximum attainable, in the effort to increase the efficiency of the steam engine and in the application of steam to new purposes, cannot be to-day predicted, or even, so far as the writer can see, imagined. High pressures like those adopted by Perkins and by Alban may yet be found useful. It was therefore proposed to carry out the tables to be constructed far beyond the limit of present necessities.

It was further proposed to ascertain the weights of steam and of water contained in each of the more common forms of steam boilers, and to determine the total and relative amounts of energy confined in each under the usual conditions of working in every-day practice, and thus to ascertain their relative destructive power in case of explosion. This part of the work is reserved for description in a succeeding section of this paper. The present section is devoted to the first part of the subject.

At the commencement of this work, the writer employed the late Mr. W. G. Cartwright, M. E., as computer, and, with his aid, prepared tables extending from 50 pounds per square inch to 100, at intervals of ten pounds, up to 250 with intervals of 25 pounds, then 300, and up to 1,000 pounds per square inch by 100 pounds, and with larger intervals up to 10,000 and 20,000 pounds. The available energy of the heated water was computed, the energy obtainable from the so-called "latent heat," and their sum, *i.e.*, the available energy of steam per unit of weight. In the course of this work, each figure was calculated independently by two computers, and thus checked. As a further check, the figures so obtained were plotted, and the curve representing the law of their variation was drawn. This was a smooth curve of moderate curvature and an incorrect determination was plainly revealed, and easily detected, by falling outside the curve. Three curves were thus constructed which will be given later: (1) the Curve of Available Energy of Heated Water; (2) the Curve of Available Energy of Latent Heat; (3) the Curve of Available Energy of Steam. The second of these curves presents an interesting peculiarity which will be pointed out when studying the forms of the several curves and the tables of results.

The work was interrupted by more pressing duties, and was finally resumed in the spring of 1884 and completed in the form now presented. The computers of the more complete tables here given were Messrs. Ernest H. Foster, M. E., and Kenneth Torrance, M. E., who, pursuing the same method as was originally adopted for the earlier computations, have revised the whole work, recalculating every figure, extending the tables by interpolation, and carrying them up to a still higher pressure than was originally proposed. The tables here presented range from 20 pounds per square inch, (1·4 kgs. per sq. cm.) up to 100,000 pounds per square inch (7,030.83 kgs. per sq. cm.) the maximum probably falling far beyond the range of possible application, its temperature exceeding that at which the metals retain their tenacity, and, in some cases, exceeding their melting points. These high figures are not to be taken as exact. The relation of temperature to pressure is obtained by the use of Rankine's equation, of which it can only be said that it is wonderfully exact throughout the range of pressures within which experiment has extended, and within which it can be verified. The values estimated and tabulated are probably quite exact enough for the present purposes of even the military engineer and ordnance officer. The form of the equation, and of the curve representing the law of variation of pressure with temperature, indicates that, if exact at the familiar pressures and temperatures, it is not likely to be inexact at higher pressures. The curve, at its upper extremity, becomes nearly rectilinear.

The table which follows presents the values of the pressures in pounds per square inch above a vacuum, the corresponding reading of the steam gauge (allowing a barometric pressure of 14·7 pounds per square inch), the same pressures reckoned in atmospheres, the corresponding temperatures as given by the Centigrade and the Fahrenheit thermometers, and as reckoned both from the usual and the absolute zeros. The amount of the explosive energy of a unit weight of water, of the latent heat in a unit weight of steam, and the total available heat energy of the steam, are given for each of the stated temperatures and pressures throughout the whole range in British measures, atmospheric pressures being assumed to limit expansion. The values of the latent heats are taken from Regnault, for moderate pressures, and are calculated for the higher pressures, beyond the range of experiment, by the use of Rankine's modification of Regnault's formula.

Table I.—Available Stored Energy in Water and Steam.

Pressure above a vacuum in pounds per square inch.	Same pressure as indicated by steam gauge, allowing 14·7 lbs. for atmospheric pressure.	Absolute pressure in atmospheres.	Number of British Thermal units required for the evaporation of one pound of water, known as latent heat of evaporation, H.	Temperature in degrees Fahrenheit of the steam and of the water from which it is evaporated.	Temperature in degrees Centigrade of the steam and of the water from which it is evaporated.	Corresponding absolute temperature in degrees Fahrenheit.	Corresponding absolute temperature in degrees Centigrade.	Amount of energy contained in one pound of water which may be liberated by explosion or expansion to 212° Fahr.	Corresponding am't of energy contained in the latent heat of evaporation.	Total amount of energy contained in one pound of steam at corresponding temperatures and pressures.
20	5·3	1·36	954·415	227·9	108·8	689·0	382·8	145·9	16872·9	17018·8
25	10·3	1·70	945·825	240·0	115·5	701·2	389·5	439·7	29156·8	29596·5
30	15·3	2·04	938·925	250·2	121·2	711·4	395·2	813·5	38921·9	39735·4
35	20·3	2·38	932·1523	259·1	126·1	720·3	400·1	1223·4	47054·9	48278·3
40	25·3	2·72	926·4728	267·1	130·1	728·3	404·6	1645·7	54111·7	55757·4
45	30·3	3·06	921·3343	274·2	134·5	735·4	408·5	2112·9	60158·1	62271·0
50	35·3	3·40	916·6316	280·8	138·2	742·0	412·2	2550·4	65613·8	68164·2
55	40·3	3·74	912·2906	286·8	141·5	748·0	415·5	2999·9	70428·7	73428·6
60	45·3	4·08	908·2472	292·5	144·7	753·7	418·7	3449·2	74884·6	78333·8
65	50·3	4·42	904·4621	297·7	147·6	758·9	421·6	3899·8	78850·5	82750·3
70	55·3	4·76	900·8991	302·7	150·4	763·9	424·4	4361·1	82577·7	86938·8
75	60·3	5·10	897·5269	307·3	152·9	768·5	426·9	4815·8	85923·6	90739·4
80	65·3	5·44	894·3304	311·8	155·4	773·0	429·4	5206·5	89138·7	94345·2
85	70·3	5·78	891·2862	316·0	157·7	777·2	431·7	5638·9	92073·3	97712·2
90	75·3	6·12	888·3758	320·0	160·0	781·2	434·0	6058·1	94814·7	100872·8
95	80·3	6·46	885·5887	323·8	162·1	785·0	436·1	6474·2	97447·2	103921·4
100	85·3	6·80	882·9144	327·5	164·1	788·7	438·1	6885·2	99787·6	106672·8
105	90·3	7·14	880·3429	331·1	166·1	792·3	440·1	7290·3	102163·3	109453·6
110	95·3	7·48	877·8653	334·5	168·0	795·7	442·0	7689·0	104334·9	112023·9
115	100·3	7·82	875·4721	337·8	169·8	799·0	443·8	8087·3	106421·7	114509·0
120	105·3	8·16	873·1555	340·9	171·6	802·1	445·6	8483·1	108325·4	116808·5
125	110·3	8·50	870·9115	344·0	173·3	805·2	447·3	8864·9	110219·9	119084·8
130	115·3	8·84	868·7351	347·0	175·0	808·2	449·0	9252·6	112025·6	121278·2
135	120·3	9·18	866·6223	349·9	176·6	811·1	450·6	9627·0	113745·7	123372·7
140	125·3	9·52	864·5661	352·7	178·1	813·9	452·1	9992·6	115382·1	125374·7
145	130·3	9·86	862·5679	355·5	179·7	816·7	453·7	10361·0	117003·5	121364·5
150	135·3	10·20	860·6213	358·1	181·1	819·3	455·1	10536·5	118477·2	129003·7
155	140·3	10·54	858·7276	360·7	182·6	821·9	456·6	11085·9	119939·4	131025·3
160	145·3	10·88	856·8740	363·2	184·0	824·4	458·0	11444·2	121323·6	132767·8

TABLE I.—AVAILABLE STORED ENERGY IN WATER AND STEAM.—*Continued.*

Pressure above a vacuum in pounds per square inch.	Same pressure as indicated by steam gauge, allowing 14·7 lbs. for atmospheric pressure.	Absolute pressure in atmospheres.	Number of British Thermal units required for the evaporation of one pound of water, known as latent heat of evaporation, H.	Temperature in degrees Fahrenheit of the steam and of the water from which it is evaporated.	Temperature in degrees Centigrade of the steam and of the water from which it is evaporated.	Corresponding absolute temperature in degrees Fahrenheit.	Corresponding absolute temperature in degrees Centigrade.	Amount of energy contained in one pound of water which may be liberated by explosion or expansion to 212° Fahr.	Corresponding am't of energy contained in the latent heat of evaporation.	Total amount of energy contained in one pound of steam at corresponding temperatures and pressures.
165	150·3	11·22	855·0654	365·7	185·4	826·9	459·4	11823·4	122697·8	134521·2
170	155·3	11·56	853·2912	368·1	186·7	829·3	460·7	12141·3	123995·5	136136·8
175	160·3	11·90	851·5670	370·5	188·0	831·7	462·0	12508·7	125284·7	137793·4
180	165·3	12·21	849·8698	372·8	189·3	834·0	463·3	12821·4	126499·1	139320·5
185	170·3	12·58	848·2086	375·0	190·5	836·2	464·5	13182·0	127642·4	140824·4
190	175·3	12·92	846·5841	377·2	191·7	838·4	465·7	13367·1	128778·8	142145·9
195	180·3	13·26	844·9938	379·4	193·0	840·6	467·0	13844·1	129908·3	143752·4
200	185·3	13·60	843·4326	381·5	194·1	842·7	468·1	14153·3	130967·4	145120·7
210	195·3	14·28	840·3967	385·6	196·4	846·8	470·4	14830·8	133003·2	147834·0
220	205·3	14·66	838·5864	389·8	198·7	851·0	472·7	15463·1	136003·3	151466·4
230	215·3	15·64	833·9691	394·2	201·2	855·4	475·2	16180·3	157134·2	153314·7
240	225·3	16·32	832·6419	397·9	203·3	859·1	477·3	16790·2	139094·3	155884·5
250	235·3	17·00	850·3630	401·0	205·0	862·2	479·0	17314·4	140516·0	157830·4
500	485·3	34·01	780·8592	467·6	242·0	898·8	516·0	30055·5	165892·7	195948·2
1000	985·3	68.02	720·4350	546·8	286·0	1008·0	560·0	48671.5	179212·0	227883·5
2000	1985·3	136·05	643·9049	643·7	339·8	1101·9	613·8	75777·2	194221·3	269998·5
3000	2985·3	204·08	590·8038	708·3	375·7	1169·5	649·7	96116·3	193555·0	289671·3
4000	3985·3	272·10	544·6774	763·7	405·9	1223·9	679·9	114498·6	189201·7	303700·3
5000	4985·3	340·13	505·7339	807·8	431·1	1269·0	705·1	130494·2	183305·9	313800·1
6000	5985·3	408·16	471·8473	845·9	452·1	1307·1	726·1	144413·4	176656·9	321070·3
7000	6985·3	486·19	439·9985	881·2	471·7	1342·4	745·7	157944·2	169333·3	327277·5
8000	7985·3	544·21	409·4533	914·3	490·1	1375·5	764·1	170832·0	161392·6	332224·6
9000	8985·3	612·21	382·6347	945·2	507·3	1406·4	781·5	183998·6	153998·0	337142·6
10000	9985·3	680·27	355·2491	971·4	521·8	1432·6	795·8	193787·0	145376·8	339164·2
100000	99985·3	6802·72	305·3040	2163·4	1184·1	2624·6	1458·1			

Studying the table, the most remarkable fact noted at the lower pressures is the enormous difference in the amounts of energy, in available form, contained in the water and in the steam, and between the energy of sensible heat and that of latent heat, the sum of which constitutes the total energy of the steam. At 20 pounds per square inch above zero (1·36 atmos.), the water contains but 145·9 foot-pounds per pound; while the latent heat is equivalent to 16,872.9 foot-pounds, or more than 115 times as much; *i. e.*, the steam contains 116 times as much energy in the form of heat per pound, as does the water, from which it is formed, at the same temperature. The temperature is low; but the amount of energy expended in the production of the molecular change resulting in the conversion of the water into steam is very great, in consequence of the enormous expansion then taking place. At 50 pounds, the ratio is 20 to 1; at 100 pounds per square inch, it is 14 to 1, at 500 it is 5 to 1; while at 5,000 pounds the energy of latent heat is but 1·4 that of the sensible heat. The two quantities become equal at about 7,500 pounds. At the highest temperature and pressure tabled, the same law would make the latent heat negative; it is of course uncertain what is the fact at that point.

At 50 pounds per square inch, the energy of heated water is 2550·4 foot-pounds, while that of the steam is 68,184, or enough to raise its own weight to a height in each case of a half mile or of 12 miles. At 75 pounds the figures are 4,816 and 90,739, or equivalent to the work demanded to raise the unit weight to a height of four-fifths, and of about 17 miles, respectively. At 100 pounds the heights are over one mile for the water, and above 20 miles for the steam.

Plotting the tabulated figures and determining the form of the curve representing the law of variation of each set, we obtain the peculiar set of diagrams exhibited in the accompanying engraving. In figure 1 are seen the curves of absolute temperature and of latent heat as varying with variation of pressure. They are smooth and beautifully formed lines, having no relation to any of the familiar curves of the text-books on co-ordinate geometry. In figure 2 are given the curves of available energy of the water, of latent heat, and of steam. The first and third have evident kinship with the two curves given in the preceding illustration; but the curve of energy of latent heat is of an entirely different kind, and is not only peculiar in its variation in radius of curvature, but also in the fact of presenting a maximum ordinate at an early point in its course. This maximum is

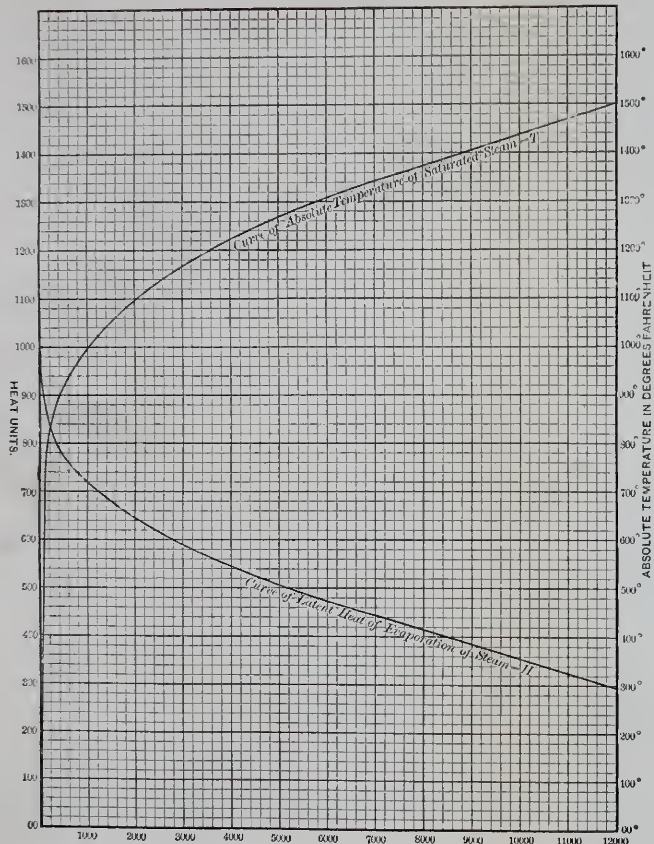
Curve of Absolute Temperature of Saturated Steam—T
Curve of Latent Heat of Evaporation of Steam—H
HEAT UNITS.
ABSOLUTE TEMPERATURE IN DEGREES FAHRENHEIT

FIG. 1.

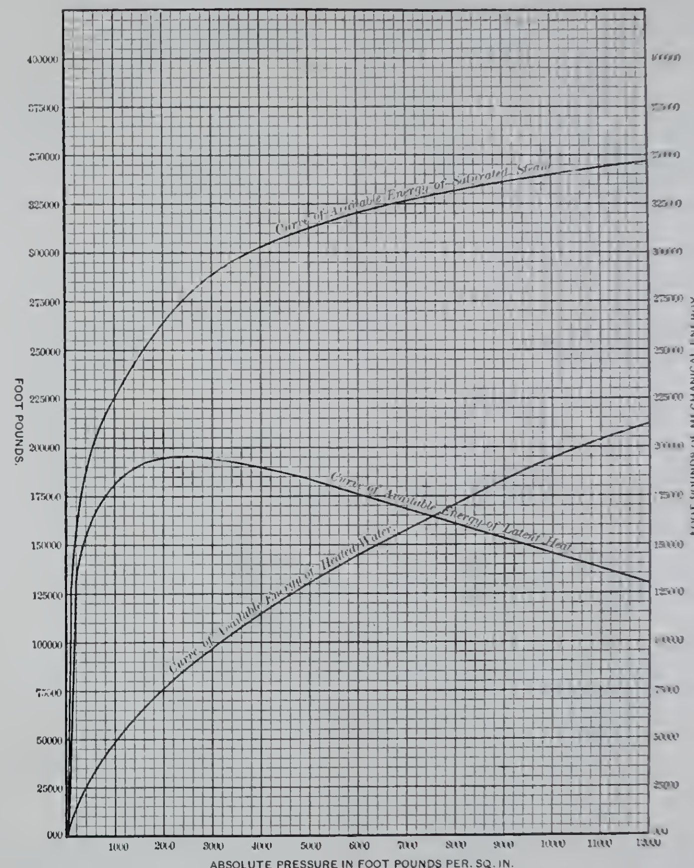
Curve of Available Energy of Saturated Steam
Curve of Available Energy of Latent Heat
Curve of Available Energy of Heated Water
FOOT POUNDS.
FOOT POUNDS OF MECHANICAL ENERGY
ABSOLUTE PRESSURE IN FOOT POUNDS PER. SQ. IN.

FIG. 2.

found at a pressure of about one ton per square inch, a pressure easily attainable by the engineer.

Examining the equations of those curves it is seen that they have no relation to the conic sections, and that the curve, the peculiarities of which are here noted, is symmetrical about one of its abscissas, and that it must have, if the expression holds for such pressures, another point of contrary flexure at some enormously high pressure and temperature. The formula is not, however, a "rational" one, and it is by no means certain that the curve is of the character indicated; although it is exceedingly probable that it may be. The presence of this characteristic point, should experiment finally confirm the deduction here made, will be likely to prove interesting, and it may be important; its discovery may possibly prove to be useful. (See Fig. 2.)

The curve of energy of steam is simply the curve obtained by the superposition of one of the two preceding curves upon the other. It rises rapidily at first, with increase of temperature, then gradually rises more slowly, turning gracefully to the right, and finally becoming nearly rectilinear. The curve of available energy of heated water, exhibits similar characteristics; but its curvature is more gradual and more uniform.

Comparing the energy of water and of steam in the steam boiler, with that of gunpowder, as used in ordnance, it will be found that, at high pressures, the former become possible rivals of the latter. The energy of gunpowder is somewhat variable with composition and perfection of manufacture, and is very variable in actual use, in consequence of the losses in ordnance due to leakage, failure of combustion, or retarded combustion in the gun. Taking its value at what the writer would consider a fair figure, 250,000 foot-pounds per pound, it is seen that, as found by Airy, a cubic foot of heated water, under a pressure of 60 or 70 pounds per square inch, has about the same energy as one pound of gunpowder. The gunpowder exploded has energy sufficient to raise its own weight to a height of nearly 50 miles; while the water has enough to raise its weight about one-sixtieth that height At a low red heat, water has about 40 times this latter amount of energy in a form to be so expended. One pound of steam, at 60 pounds pressure, has about one-third the energy of a pound of gunpowder. At 100 pounds it has as much energy as two-fifths of a pound of powder, and at higher pressures, its energy increases very slowly.

Section II.—Explosive Energy of Boilers.

In illustration of the results of application of the computations which have been given in the preceding section of this paper, and for the purpose of obtaining some idea of the amount of destructive energy stored in steam-boilers of familiar forms, such as the engineer is constantly called upon to deal with, and such as the public are continually endangered by, Table II, has been calculated. This table is made up, with the assistance of Professor C. A. Carr, from notes of dimensions of boilers designed, or managed, at various times, by the writer, or in other ways having special interest to him. They include nearly all of the forms in common use, and are representative of familiar and ordinary practice.

No. 1 is the common, simple, plain cylindrical boiler. It is often adopted when the cheapness of fuel or the impurity of the water-supply renders it unadvisable to use the more complex, though more efficient, kinds. It is the cheapest and simplest in form of all the boilers. The boiler here taken was designed by the writer many years ago for a mill so situated as to make this the best form for adoption, and for the reasons above given. It is thirty inches in diameter, thirty feet long, and is rated at ten HP., although such a boiler is often forced up to double that capacity. The boiler weighs a little over a ton, and contains more than twice its weight of water. The water, at a temperature corresponding to that of steam at 100 pounds pressure per square inch, contains over 46,600,000 foot-pounds of available explosive energy, while the steam, which has but one-fifth of one per cent. of the weight of the water, stores about 1,300,000 foot-pounds, giving a total of 48,000,000 foot-pounds, nearly, or sufficient to raise one pound nearly 10,000 miles. This is sufficient to throw the boiler 19,000 feet high, or nearly four miles, and with an initial velocity of projection of 1,111 feet per second.

Comparing this with the succeeding cases, it is seen that this is the most destructive form of boiler on the whole list. Its simplicity and its strength of form make it an exceedingly safe boiler, so long as it is kept in good order and properly managed; but, if through phenomenal ignorance or recklessness on the part of proprietor or attendant, the boiler is exploded, the consequences are usually exceptionally disastrous.

The explosion of a boiler of this form and of the proportions here

TABLE II.—*Stored Energy of Steam Boilers.*

Type.	Area of G.S.	Area of H. S.	Pressure lbs. per sq. in.	Rated power H. P.	Weight of Boiler.	Weight of Water.	Weight of Steam.	Stored energy in (available). Water.	Stored energy in (available). Steam.	Stored energy in (available). Total.	Energy per lb. of Boiler	Energy per lb. of Tot w't	Max h't of proj'tn Boiler	Max h't of proj'tn T'tl	Initial velocity. Boiler	Initial velocity. Total
	Sq. feet.				Lbs.			Foot lbs.			Foot lbs		Feet.		Ft. per sec.	
1. Plain Cylinder.....	15	120	100	10	2500	5764	11·325	46,605,200	1,297,380	47,902,580	19161	5879	19161	5879	1111	615
2. Cornish Cylinder..................	36	730	30	60	16950	27471	31·45	57,570,750	1,958,420	59,509,170	3511	1336	3511	1336	476	293
3. Two Flue Cylinder..............	20	400	150	35	6775	6810	37·04	80,572,050	4,514,730	85,116,780	12563	6235	12563	6253	900	634
4. Plain Tubular........................	30	851·97	75	60	9500	8255	20·81	50,008,790	2,102,190	52,110,980	5485	2932	5485	2932	595	435
5. Locomotive...........................	22	1070	125	525	19400	5260	21·67	52,561,075	2,716,875	55,277,950	2819	2239	2819	2239	427	380
6. "	30	1350	125	650	25000	6920	31·19	69,148,790	3,910,450	73,059,240	2922	2287	2922	2287	434	384
7. "	20	1200	125	600	20565	6450	25·65	64,452,270	3,225,870	67,678,140	3291	2503	3291	2503	460	401
8. "	15	875	125	425	14020	6330	19·02	64,253,160	2,384,630	66,637,790	4753	3259	4753	3259	519	458
9. Scotch Marine.....	32	768	75	300	27045	11765	29·8	71,272,370	3,006,060	74,278,430	2747	1913	2747	1913	421	350
10. "	50·5	1119·5	75	350	37972	17730	47·2	107,408,340	4,761,210	112,169,550	2994	2012	2994	2012	439	360
11. Flue and Return Tubular...	72·5	2324	30	200	56000	42845	69·81	90,531,490	4,347,140	94,878,610	1694	959	1694	959	330	248
12. " " " " ...	72	1755	30	180	56000	48570	73·07	102,628,410	4,550,140	107,178,550	1914	1024	1914	1024	351	257
13. Water Tube.......	70	2806	100	250	34450	21325	35·31	172,455,270	4,043,310	176,498,580	5123	3165	5123	3165	575	451
14. "	100	3000	100	250	45000	28115	58·5	2-7,366,000	6,719,980	234,085,980	5202	3177	5202	3177	579	452
15. "	100	3000	100	250	54000	13410	23·64	108,346,670	2,606,990	110,935,660	2051	1615	2051	1615	361	326

given, in the year 1843, in the establishment of Messrs. R. L. Thurston & Co., at Providence, R. I., through mismanagement, is well remembered by the writer. The boiler-house was entirely destroyed, the main building seriously damaged, and a large expense was incurred in the purchase of new tools to replace those destroyed. No lives were lost, as the explosion fortunately occurred after the workmen had left the building. A similar explosion of a boiler of this size occurred some years later, within sight of the writer, which drove one end of the exploding boiler through a 16-inch wall, and several hundred feet through the air, cutting off an elm tree high above the ground, where it measured 9 inches in diameter, partly destroying a house in its further flight, and fell in the street beyond, where it was found *red hot* immediately after striking the earth. Long after the writer reached the spot, although a heavy rain was falling, it was too hot to be touched and was finally, nearly two hours later, cooled off by a stream of water from a hose, in order that it might be moved and inspected. It had been overheated, in consequence of low water, and cold feed had then been turned into it. The boiler was in very good order, but four years old, and was considered safe for 110 pounds. The engineer was seriously injured, and a pedestrian passing at the instant of the explosion was buried in the ruins of the falling walls and killed. The energy of this explosion was very much less than that stored in the boiler when in regular work.

No. 2 was a "Cornish" boiler designed by the writer, about 1860, and set to be fired under the shell. It was 6 feet by 36, and contained a 36-inch flue. The shell and flue were both of iron $\frac{3}{8}$-inch in thickness. The boiler was tested up to 60 pounds, at which pressure the flue showed some indications of alteration of form. It was strengthened by stay-rings, and the boiler was worked at 30 pounds. The boiler contained about 12 tons of water, weighed itself $7\frac{1}{2}$ tons, and the volume of steam in its steam space weighed but $31\frac{1}{2}$ pounds. The stored available energies were about 57,600,000 foot-pounds, and about 2,000,000 of foot-pounds, in the water and steam, respectively, a total of nearly 60,000,000. This was sufficient to throw the boiler to the height of 3,500 feet, or over three-fifths of a mile.

Comparing this with the preceding, it is seen that the introduction of the single flue, of half the diameter of the boiler, and the reduced pressure, have reduced the relative destructive power to but little more than one-sixth that of the preceding form.

No. 3 is a "two-flue" or Lancashire boiler, similar in form and in proportions to many in use on the steamboats plying on our Western rivers, and which have acquired a very unenviable reputation by their occasional display of energy when carelessly handled. That here taken in illustration was designed by the writer, 42 inches in diameter, with two 14-inch flues of $\frac{3}{8}$ iron, and is here taken as working at a pressure, as permitted by law, of 150 pounds per square inch. It is rated at 35 horse-power, but such a boiler is often driven far above this figure. The boiler contains about its own weight, 3 tons, of water, and but 37 pounds of steam. The stored available energy is 85,000,000 foot-pounds, of which the steam contains but a little above five per cent. Its explosion would uncage sufficient energy to throw the boiler nearly $2\frac{1}{2}$ miles high, with an initial velocity of 900 feet per second. Both this boiler and the plain cylinder are thus seen to have a projectile effect only to be compared to that of ordnance.

A boiler of this class, which the writer was called upon to inspect after explosion, had formed one of a "battery" of ten or twelve, and was set next the outside boiler of the lot. Its explosion threw the latter entirely out of the boiler-house into an adjoining yard, displaced the boiler on the opposite side, and demolished the boiler-house completely. The exploding boiler was torn into many pieces. The shell was torn into a helical ribbon, which was unwound from end to end. The furnace end of the boiler flew across the space in front of its house, tore down the side of a "kier-house," and demolished the kiers, nearly killing the kier-house attendant, who was standing between two kiers. The opposite end of the boiler was thrown through the air, describing a trajectory having an altitude of fifty feet, and a range of several hundred, doing much damage to property *en route*, finally landing in a neighboring field. The furnace front was found by the writer on the top of a hill, a quarter of a mile, nearly, from the boiler-house. The fireman, who was on the top of the boiler at the instant of the explosion, endeavoring to open a steam connection to relieve the boiler, then containing an excess of steam and a deficiency of water, was thrown over the roof of the mill, and his body was picked up in the field on the other side, and carried away in a packing-box measuring about two feet on each side. Cause: low water and consequent overheating, and the introduction of feed before hauling fires and cooling down. The energy expended was much less than that calculated as above.

No. 4 is the common plain tubular boiler, substantially as designed

by the writer at about the same time with those already described, and of the same in dimensions as that adopted as a standard by the Hartford Steam Boiler Insurance Co.* It is a favorite form of boiler, and deservedly so, in the opinion of the writer, with all makers and users of shell boilers. That here taken is 60 inches in diameter, containing 66 3-inch tubes, and is 15 feet long. The general testimony of the best designers of this type, so far as the writer has been able to obtain definite opinions, as well as the observation and the experience of the writer himself, indicate that these proportions are usually thoroughly satisfactory. A length of tube of from 50 to 60 diameters, and liberal spacing, seem to be especially advantageous. The specimen here chosen has 850 feet of heating and 30 feet of grate surface, is rated at 60 horse-power, but is oftener driven up to 75, weighs 9,500 pounds, and contains nearly its own weight of water, but only 21 pounds of steam, when under a pressure of 75 pounds per square inch, which is below its safe allowance. It stores 52,000,000 foot-pounds of energy, of which but 4 per cent. is in the steam, and this is enough to drive the boiler just about one mile into the air, with an initial velocity of nearly 600 feet per second. The common upright tubular boiler may be classed with No. 4.

Nos. 5–8 are two of the Baldwin and two of the Cooke locomotive boilers, of which drawings and weights are furnished by the builders. They are of different sizes and both freight and passenger engines. The powers are probably rated low. They range from 15 to 50 square feet in area of grate, and from 875 to 1,350 square feet of heating surface. In weight, the range is much less, running from 2½ to a little above 3 tons of water, and from 20 to 30 pounds of steam, assuming all to carry 125 pounds pressure. The boilers are seen to weigh from 2½ to 3 times as much as the water. These proportions differ considerably from those of the stationary boilers which have been already considered. The stored energy averages about 70,000,000 foot-pounds, and the heights and velocities of projection not far from 3,000 and 500 feet; although, in one case, they become nearly one mile, and 550 feet respectively. The total energy is only exceeded, among the stationary boilers, by the two-flued boiler at 150 pounds pressure.

The violence of the explosion of the locomotive is naturally most terrible, exceeding, as it does, that of ordnance fired with a charge of 150 pounds of powder of best quality, or perhaps 250 pounds of ordi-

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* *The Locomotive.*, September, 1884.

nary quality fired in the usual way.* On the occasion of such an explosion which the writer was called upon to investigate, in the course of his professional practice, the engine was hauling a train of coal cars weighing about 1,000 tons. The steam had been shut off from the cylinders a few minutes before, as the train passed over the crest of an incline and started down the hill, and the throttle again opened a few moments before the explosion. The explosion killed the engineer, the fireman, and a brakeman, tore the fire-box to pieces, threw the engine from the track, turning it completely around, broke up the running parts of the machinery, and made very complete destruction of the whole engine. There was no indication, that the writer could detect, of low water; and he attributed the accident to weakening of the fire-box sheets at the lower parts of the water-legs by corrosion. The use of water-grates, the insertion of which produced some loss of strength at the fire-box, may have had something to do with it, however. The bodies of the engineer and fireman were found several hundred feet from the wreck, the former among the branches of a tree by the side of the track. This violence of projection of smaller masses would seem to indicate the concentration of the energy of the heat stored in the boiler, when converted into mechanical energy, upon the front of the boiler, and its application largely to the impulsion of adjacent bodies. The range of projection was, in one case, fully equal to the calculated range. The energy expended is here the full amount calculated.

Nos. 9 and 10 are marine boilers of the Scotch or "drum" form. These boilers have come into use by the usual process of selection, with the gradual increase of steam pressures occurring during the past generation as an accompaniment of the introduction of the compound engine and high ratios of expansion. The selected examples are designed for use in the new vessels of the U. S. Navy. The dimensions are obtained from the Navy Department, as figured by the Chief Draughtsman, Mr. George B. Whiting. The first is that designed for the *Nipsic* the second for the *Despatch*. They are of 300 and 350 horse-power, and contain, respectively, 74,000,000 and 112,000,000 of foot-pounds of available energy, or about 3,000 foot-pounds per pound of boiler, and sufficient to give a height and velocity of projection of 3,000 and above 400 feet. These boilers are worked at a lower

* The theoretical effect of good gunpowder is about 500 foot-tons per pound, according to Noble and Abel.

pressure than locomotive boilers; but the pressure is gradually and constantly increasing from decade to decade, and the amount of explosive energy carried in our modern steam vessels is thus seen to be already equal to that of our locomotives, and in some cases already considerably exceeds that which they would carry were they supplied with boilers of the locomotive type and worked at locomotive pressures. The explosion of the locomotive boiler endangers comparatively few lives and seldom does serious injury to property, outside the engine itself. The explosion of one of these marine boilers while at sea would be likely on to be destructive of many lives, if not of the vessel itself and all board.

Nos. 11 and 12 are boilers of the older types such as are still to be seen in steamboats plying upon the Hudson and other of our rivers, and in New York harbor and bay. No. 11 is a return-tubular boiler having a shell 10 feet in diameter by 23 feet long, 2 furnaces each $7\frac{1}{2}$ feet deep, 8 15-inch and 2 9-inch flues, and 85 return tubes, $4\frac{1}{2}$ inches by 15 feet. The boiler weighs 25 tons, contains nearly 20 tons of water and 70 pounds of steam, and at 30 pounds pressure stores 95,000,000 foot-pounds of available energy, of which 5 per cent. resides in the steam. This is enough to hoist the boiler one-third of a mile, with a velocity of projection of 330 feet per second. The second of these two boilers is of the same weight, also of about 200 horse-power, but carries a little more water and steam and stores 107,000,000 foot-pounds of energy, or enough to raise it 1,900 feet. This was a return-flue boiler, 33 feet long and having a shell $8\frac{3}{4}$ feet in diameter, flues $8\frac{1}{2}$ to 15 inches in diameter, according to location. These boilers were designed, years ago, by Messrs. Fletcher & Harrison (now the W. & A. Fletcher Co.) of New York City. It was a boiler of the return-flue variety, to which that just described belongs, that exploded in the *Westfield* ferry boat, July 30th, 1871, causing the death of about 100 persons and wounding as many more. The writer was employed to investigate the case for the officials upon whom the duty was legally and technically incumbent. It was found that the cause of the explosion was the extensive corrosion of one of the girth seams of the shell. The accident occurred when the pressure was about that ordinarily carried, and considerably less than that at which the boiler had been tested but a short time before. The energy liberated was therefore about the same as would be calculated as above from the known dimensions and capacity of the boiler. The destruction of the boiler itself,

its displacement, and the destruction of that part of the boat adjacent to it, were minor effects of the accident.*

A boiler of the return-tubular class was tested to the bursting point, under steam, by Mr. F. B. Stevens, at Sandy Hook, November, 1871. The water was up to the water-line, and the energy liberated was thus the full amount calculated. As then reported by the writer,† "when a pressure of 50 pounds was reached, a report was heard which was probably caused by the breaking of one or more braces, and at 53½ pounds, the boiler was seen to explode with terrible force. The whole enclosure was obscured by the vast masses of steam liberated; the air was dotted with the flying fragments, the largest of which, the steam drum, rising to a height variously estimated at from 200 to 400 feet, fell at a distance of 450 feet from its original position. The sound of the explosion resembled that of a heavy cannon. The boiler was torn into many pieces, and comparatively few fell back upon their original position." This boiler had been tested by hydrostatic pressure, before its explosion, up to a pressure exceeding by 5½ pounds that at which the explosion occurred.

The writer subsequently calculated the amount of total energy stored in this boiler and analyzed the effects of the explosion, coming to the conclusions: ‡

"(1.) That it is very certain that the energy of this explosion, and all of its tremendous effects, were principally due to the simple expansion of a mass of steam suddenly liberated, at a moderate pressure, by the general disruption of a boiler of very uniform but feeble strength.

"(2.) That in this case, the liberation of the steam through out the mass of water contained in the boiler, and which took place by the evaporation of one pound in every thirteen of the water, and which resulted in setting free nearly 70,000 cubic feet of steam, would not seem to have taken place so promptly as greatly to intensify the effects of the explosion.

"(3.) It would seem very doubtful whether Zerah Colburn's hypothesis, which explains the violent ruptures of steam boilers by the supposition that the steam liberated from the mass of water, in cases of explosion, carries with it and violently projects against those parts of the shell immediately adjacent to the point of primary rupture,

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* Journal of the Franklin Institute, September 1871, R. H. Thurston.

† Journal of the Franklin Institute, January, 1872.

‡ Journal of the Franklin Institue, Feb., 1872.

large quantities of water, which, by their impact, extend the break and increase the destructive effect, can have had an illustration in the case under consideration."

"We have no right to conclude that such an action as Colburn described may not occur in many cases of explosion; on the contrary, the simple experiment described in all text-books on natural philosophy, in which water in a closed vessel, and near the boiling point, is caused to enter into violent ebullition by the reduction pressure following the application of cold to the upper part of the vessel exhibits very plainly the probability of an action taking place such as Colburn describes." . . . . "There can hardly be a doubt that cases do occur in which the same action greatly increases the destructive effect of boiler explosions"

The more recent experiments of Mr. Lawson at Pittsburg seem to the writer to indicate very strongly, if not absolutely to prove, that the Colburn theory has a foundation in fact, and that "not only may explosions be intensified in violence, but that they may be precipitated, by the action of the stored energy of the water contained in the boiler." It is probably the conviction of the majority of engineers, familiar with steam boilers, that the danger is pretty nearly proportional to the weight of water present. The boiler exploded at Sandy Hook, as above, weighed 40,000 pounds, contained 30,000 pounds of water and 150 pounds of steam, stored over 2,500,000 of thermal units, measured from the boiling point up to 300° Fahr., equivalent to above 2,000,000,-000 foot-pounds of mechanical energy, or enough to arise the whole mass more than five miles. Of this only a fraction was available, however, as shown in Table I.

The last three boilers on the list in Table II., are of a type which has come into common use only during the last 10 or 15 years. They are water-tube boilers, and all of what are popularly known as the "sectional," or "safety" class. Where a boiler is exploded, the disruption may be either general, as in some of the cases cited above, or it may be local, affecting only a limited portion of the structure. It is evident that the localization of the injury is desirable as a means of limiting the rate of discharge of the stored available energy, and thus reducing the damage resulting from the accident. It was pointed out as long ago as 1805, by the greatest engineer of this country, at that time—Col. John Stevens, of Hoboken—that the construction of boilers consisting of water tubes, principally, afforded a means of securing com-

parative safety from explosions, and a patent was issued to him by the British patent office, at that date, for a boiler resembling in its general construction the modern "safety" boiler. In the specification, communicated to the office by his son, John C. Stevens, the original of which is in the hands of the writer, Col. Stevens explains this principle of subdivision of the mass of water and of steam in boilers, as a means of insuring against destructive explosion as clearly as it has ever been explained by his recent followers. All of the latter forms of boiler belonging to this class have followed the same general plan. The writer has selected the forms here described mainly because of their being most familiar to him. He has, while preparing this paper, been engaged in directing the introduction of 250 horse-power of one type, under a very large and valuable building in New York city, where he felt unwilling to take the risk of employing a shell-boiler; he has had a boiler of another of these forms under his feet, when in his lecture-room, for more than a dozen years, where the location of a shell-boiler would have been a continual source of apprehension; and he has experimented with still another of the selected forms sufficiently to feel thoroughly at home with it, and to feel the same confidence in its safety that he has in the others. Every prudent engineer is careful to keep a shell boiler well inspected and well insured, and knows that, so cared for, the risk in their use is reduced to a very insignificant quantity; yet the writer, and probably every other engineer, finds it very satisfactory to be able to feel that any boiler that he may compelled to place under a building, or where many lives may be endangered by its explosion, is so constructed that, even were explosion to occur, it would be productive of minimum and probably small damage. The writer has not hesitated, however, where great difference of cost have entered into the case, and where the boilers could be set in a separate boiler-house, to advise the use of the shell-boiler. By proper construction, and with careful management and systematic inspection, the danger and risk are reduced to a very small amount.

The "sectional" boilers are here seen to have, for 250 horse-power each, weights ranging from about 35,000 to 55,000 pounds, to contain from 15,000 to 30,000 pounds of water and from 25 to 58 pounds of steam, to store from 110,000,000 to 230,000,000 foot-pounds of energy, equal to from 2,000 to 5,000 foot-pounds per pound of boiler. The stored available energy is thus usually less than that of any of the other stationary boilers, and not very far from the amount stored,

pound for pound, by the plain tubular boiler, the best of the older forms. It is evident that their admitted safety from destructive explosion does not come from this relation, however, but from the division of the contents into small portions, and especially from those details of construction which make it tolerably certain that any rupture shall be local. A violent explosion can only come of the general disruption of a boiler and the liberation at once of large masses of steam and water.

In the year 1872, the writer, preparing the report of a committee conducting tests of steam boilers at the exhibition of the American Institute for 1871, with the approval of the committee, wrote:*

"In this class, of which there are many different kinds in the market, the water space, and frequently the steam space, of the boiler is contained in a large number of comparatively small compartments, each of which is very strong, and the explosion of which is not likely to result in that widespread destruction of property, and that great loss of life, which so frequently follows the explosion of the older and more common forms of steam boiler.

"Your committee feel confident that the introduction of this class of steam boilers will do much toward the removal of the cause of that universal feeling of distrust which renders the presence of a steam boiler so objectionable in every locality. The difficulties in inspecting these boilers thoroughly, in regulating their action, and other faults of the class, are gradually being overcome, and the committee look forward with confidence to the time when their use will become general, to the exclusion of the older and more dangerous forms of boilers."

The writer is confident that this is still the sentiment of engineers generally, and the time to which that committee then looked forward with such interest is rapidly approaching. The figures just given and the comparisons made in this paper, may aid, somewhat, in awakening engineers to the realization of the importance of carefully considering the magnitude and the dangers of the wonderful force with which they have to deal, and to the importance of finding ways of making its use satisfactorily safe.

HOBOKEN, N. J., Oct., 1884.

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* Journal of the Franklin Institute, Feb., 1872.

GLIMPSES OF THE INTERNATIONAL ELECTRICAL EXHIBITION.

By Prof. Edwin J. Houston.

No. 2.—Dolbear's Electro-static System of Telephony.

Prominent among the numerous exhibits of telephones at the International Electrical Exhibition, was the exhibit of Prof. Amos E. Dolbear. The experiments of this gentleman in telephony were contemporaneous with those of Bell, and cover a wide field of research. Prof. Dolbear, by the introduction of several novel features, has not only greatly improved the system of magneto-electric telephony, now generally accredited to Bell, but has also invented and carried into successful, practical use, a system quite novel and distinct. We allude to his system of electro-static telephony.

The electro-static telephone of Dolbear operates on electrical principles that are radically distinct from those of the various magnetic telephones now in common use. It stands by itself as a species of telephonic apparatus, and as such possesses latent possibilities not found in magnetic telephones.

The idea of conveying articulate speech by means of electricity, like all great ideas, has not been the product of any single mind. Indeed, it appears to have occurred to many able minds long before it was so powerfully revived by the successful experiments and inventions of Bell. Nothing can illustrate the truth of this remark as forcibly as the many different claims that were brought forward during the Exhibition as to the real inventor of the telephone.

Without attempting in this paper to sketch the early inventions in telephony, it will perhaps suffice to state that the first telephone that actually transmitted intelligible, articulate speech, and that was clearly at the time of its invention claimed as able to do so, was invented by Philip Reis, in Germany, in 1860. Though considerable difference of opinion has been expressed as to the practical operation of this instrument there appears to be no doubt but that it could, and in fact did, transmit articulate speech. The success of his apparatus in accomplishing what he publicly claimed for it, would appear to be sufficient to entitle Reis to the credit of the invention.

Since the time of Reis many have claimed the telephone as their

invention. Without stopping to discuss their claims, it will suffice to say, that among them Prof. Alex. Graham Bell was one of the first to actually employ the principles laid down by Reis in his many publications on the subject, in such a manner as to permit the telephone to take the prominent commercial position it now occupies.

Prof. Dolbear was an early worker in the field of telephony. His labors appear to have been contemporaneous with those of Bell and Gray. Indeed, in the opinion of some, had he looked more carefully after his interests in the United States Patent Office, he might to-day share with Bell the credit of bringing the telephone into practical commercial use.

The early investigations of Dolbear in telephony, which we believe date from early August, 1876, resulted in the invention of the magneto-electric telephone, as distinguished from the electro-magnetic telephone; his later inventions produced his system of electro-static telephony.

In the magneto-electric telephone, the voice acting on a magnetic diaphragm moves it towards and from a coil of insulated wire wrapped around the end of a permanent magnet placed near, but not in contact with the diaphragm. These movements of the magnetized diaphragm

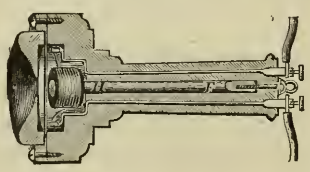

FIG. 1.—The Telephone.

towards and from the magnet pole and its insulated coil, produce electrical currents in the coil. These currents, which flow alternately in opposite directions, are caused to traverse a conducting wire and to alternately strengthen and weaken the magnetism in a similar instrument placed at the other end of the line. An increase in the strength of the magnetism causes a movement of the diaphragm of the receiving telephone towards the magnet pole; a decrease in the strength of the magnetism causes a movement of the diaphragm of the receiving telephone from the magnet pole.

Since these movements are caused by the movements of the diaphragm of the transmitting telephone towards and from the magnet pole it faces, consequent on the waves of the speakers voice falling on it, and since they produce electric currents that cause exactly similar move-

ments in the diaphragm of the receiving telephone, it follows that whatever is spoken against the transmitting telephone will be heard by an observer listening at the receiving telephone.

In order the more thoroughly to appreciate the operation of the magneto-electric telephone reference may be had to Fig. 1, in which is shown in partial longitudinal section the magneto-electric telephone receiver as generally constructed by Bell. *F*, is a straight bar magnet of steel, with its poles at its extremities. A coil of insulated wire, *H*, is placed around one end of the magnet in the position shown. Near, but not in contact with the end of the permanent magnet and its surrounding wire coil, is placed a circular plate or diaphragm of soft iron, rigidly attached at its edges, but free to move elsewhere. This diaphragm becomes magnetized by induction from the permanent magnet, and when set into vibration by the voice, produces currents of electricity in the coil *H*, that flow in one direction as the plate moves towards the coil, and in the opposite direction as it moves from it.

The manner in which these currents are utilized for the reproduction of the speaker's voice may be seen by an inspection of Fig. 2, in which

FIG. 2.—Magneto-Electric Telephone Circuit.

is shown the circuit of the ordinary magneto-electric telephone. In this case the receiving instrument is of the same construction as the transmitting instrument described in connection with Fig. 1. The circuit is represented as broken so as to convey the idea of distance.

When the diaphragm of the transmitting instrument, as for example, at *a*, moves towards the coil of insulated wire wrapped around the magnet pole, an electric current is produced which flows, we will suppose, from the coil of wire at *a*, through the ground to the other instrument, and after passing through the coil at *b*, flows over the line-wire to the other telephone. If now the direction of this current through the coil of wire in the receiving telephone is such as to increase the strength of the magnetism in the pole of the permanent magnet around which it is wrapped, its passage through the coil will be followed by a movement of the diaphragm of the receiving telephone towards its magnet pole; similarly, a movement of the diaphragm of the trans-

mitting telephone away from its magnet pole produces a current of electricity which traversing the circuit in the opposite direction, weakens the magnetism in the pole of the receiving instrument, and so permits the elasticity of its diaphragm to cause the diaphragm to move away from the magnetic pole.

The movements of the diaphragm of the transmitting telephone are therefore followed by exactly similar movements in the diaphragm of the receiving telephone. Whatever, therefore, is spoken against the diaphragm of the transmitting instrument, is reproduced by the movements of the diaphragm of the receiving instrument.

The transmitting magneto-electric telephone, therefore, is in reality a dynamo-electric machine driven by the voice of the speaker; while the receiving telephone is a dynamo-electric motor, which is so constructed as to exactly reproduce the speaker's voice.

In Dolbear's electro-static telephone the movements of the diaphragm of the receiving telephone are produced by the attractions caused by electrified bodies. The attractions of magnets are entirely done away with. The movement of the diaphragm in the opposite direction in the electro-static telephone, as in the magneto-electric telephone, is due to elasticity.

Indeed, as we will see further on, the diaphragm as well as the magnet may be dispensed with, and the observer may actually listen at the end of the line itself. In this case it is only necessary to somewhat enlarge the end of the wire.

The attractions produced when a body is electrified by friction are well known. It is attractions of this character that are utilized by Prof. Dolbear in his system of electro-static telephony.

The electricity produced by friction is, on account of its high electromotive force, well adapted to cause the attraction of light bodies. The electricity produced by means of a voltaic cell can also cause attractions of other bodies, but owing to its comparatively low electro-motive force, such attractions are not as marked as in the case of the electricity produced by friction.

Since electricity produced by friction is not as conveniently obtained as that produced by chemical action, Prof. Dolbear employs the ordinary voltaic battery in his telephone system, and raises its electromotive force to the requisite degree by the use of an induction coil, an instrument consisting essentially of a coil of short, coarse wire, called

the primary coil, surrounded by a coil of long, fine wire, called the secondary coil.

As is well known, when a battery current is sent into the primary circuit of an induction coil, a current is produced by induction in the secondary coil. This current, however, is produced only while the current in the primary is beginning, or is ceasing to flow, that is, while its strength is undergoing variations. As soon as the current through the primary coil is fairly established, and its strength is constant, all inductive effects in the secondary coil cease. When the battery current in the primary coil is broken or interrupted, an induced current is also produced in the secondary coil.

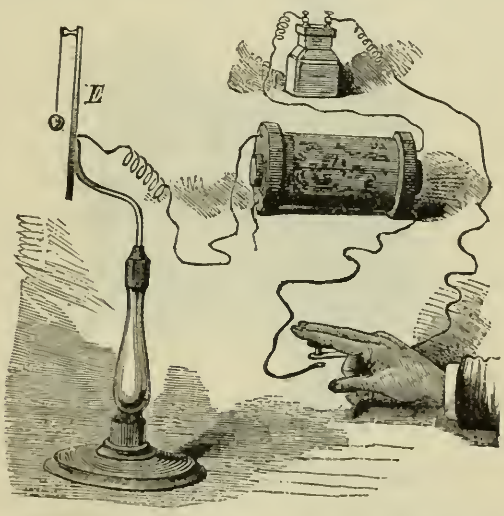

FIG. 3.—Attractions caused by a Charged Surface.

The current produced in the secondary coil by induction when the circuit of the primary coil is made or completed, flows in the opposite direction to the current in the primary; that produced in the secondary coil by the breaking of the circuit in the primary, flows in the same direction as that in the primary. Both of these induced currents possess the high electro-motive force necessary for the electro-static telephone, and are utilized by Dolbear for the reproduction of articulate speech.

When the terminals of the secondary coil are not connected, the passage of the current through the primary coil induces an electro-

motive force in the secondary that causes the potential of one terminal to be higher than that of the other, or in other words, one terminal becomes positively charged, and the other negatively charged.

If, therefore, either terminal be connected with an insulated metallic plate, such, for example, as one of the plates of an Æpinus' condenser, and a pith ball be hung before the plate in the position shown in Fig. 3, whenever the circuit of the battery is completed by depressing the key, the charge produced in the plate *E*, causes the attraction of the pith ball hung near it. Similar phenomena would be produced if the

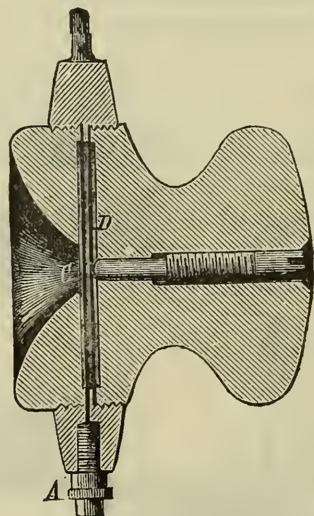

Fig. 4.—The Dolbear Electro-Static Receiver.

plate *E*, were connected with the other terminal of the secondary coil.

If, in place of the pith ball, another plate be placed near *E*, but not in electrical contact with it, and one of the plates be connected with one of the free terminals of the secondary coil, and the other plate with the other terminal, then the two plates will mutually attract each other, since one of them will receive a positive charge, and the other will receive a negative charge. If only one of the plates be free to move, the other will move toward the fixed plate, under the influence of both the charges.

Such an arrangement as that we have just described constitutes, in fact, the receiving telephone in the Dolbear system.

A form commonly given to it is shown in connection with Fig. 4.

Two metallic discs, *C*, and *D*, are mounted, as shown, so as to be near each other, but separated at their edges by some insulating substance, such, for instance, as a flange of hard rubber. The remaining portions of the plate are separated by an air space. Both plates are securely fixed at their edges, but the plate *D*, is, in addition, clamped at its middle by means of a screw. Only the plate *C*, therefore, is free to move under the influence of the electrical charges in the two plates. A suitably shaped mouth piece is placed over the plate *C*, and a conveniently shaped knob, for holding the instrument in the hand, is placed over *D*.

When the metallic discs are coated with a film of varnish or other good dielectric, they act like a Leyden jar, and may receive a permanent

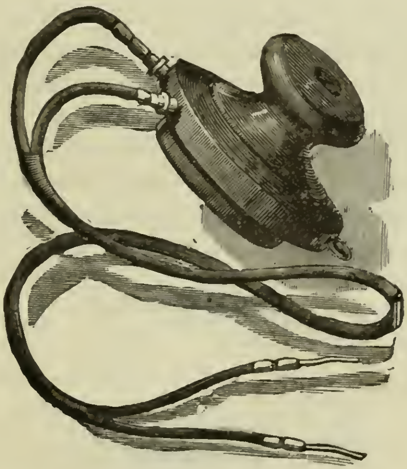

FIG. 5.—The Dolbear Electro-Static Receiver.

charge. In this condition they act like an electroscope and become exceedingly sensitive to any electrical change either of conduction or induction. Indeed, the efficiency of the electro-static telephone depends to a very great extent on this curious fact.

The receiver, therefore, is more than a mere air condenser, and depends for its efficiency upon an electrical combination thus utilized for the first time by Prof. Dolbear. When constructed and operated in this manner, the receiver becomes a transmitter, and when spoken to, transforms the vibrations of the voice into appropriate electrical vibrations, so that, from what has already been said, we may distinctly hear what is spoken, without the aid of any intermediate apparatus.

It has been found in practice that the ordinary ferrotype, photographic plates answer admirably for the purpose of an electro-static telephone receiver, since the varnish with which they are covered is superior as a dielectric, to shellac, wax, or hard rubber.

The complete receiver is shown in Fig. 5, with the separate terminals connected with each of the plates.

If one of the terminals be connected to one of the ends of the secondary coil of an induction coil, and the other end with the other terminal, then a sharp click will be heard by an observer holding the instrument to his ear, whenever the primary circuit is made or broken.

If, in place of using a key to make and break the circuit of the primary coil, an interrupter be employed capable of being operated by the voice, such, for example, as a modified Reis transmitter, then whatever is spoken into the transmitter will be heard in the receiver.

In order to transmit articulate speech distinctly, it is preferable to speak into the transmitter in a comparatively gentle tone. Under these circumstances the circuit in the primary coil is never completely broken, but the variations in the intensity of the primary current so effected, produce corresponding variations in the positive and negative charges in the opposed plates, and consequently produce, in the free plate, movements that exactly reproduce the voice.

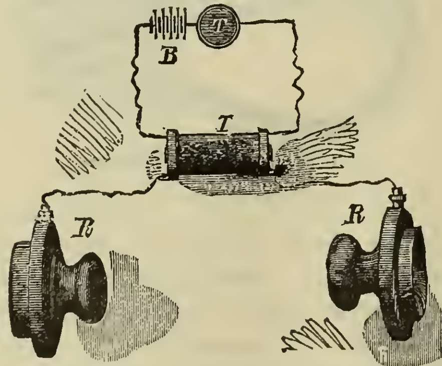

FIG. 6.—Dolbear's Singly-Connected-Plate Electro-Static Receivers.

As might be supposed, it is not necessary to connect both plates with the terminals of the induction coil. The receiver will operate if but one of the plates is so connected. In this case the other plate is connected to a metallic ring, placed on the knob, which serves as the handle of the telephone. When the hearer places his body in this manner

in electrical connection with the plate that is unconnected with the induction coil, he can hear better, because the plate can thus be the more readily oppositely electrified by induction.

It will readily be understood that in the electro-static receiving telephone it is essential that the two plates be separated by some good dielectric, or non-conducting material, since otherwise there can be no induction, and no effects of sound will be produced. Any non-conducting material between the two plates will suffice.

It is not necessary to employ the ordinary electro-static receiver, since articulate speech will be heard, if the terminals from the induction coil be suitably insulated and held to the ears of an observer, while a person is speaking into any suitable transmitter connected with the induction coil.

Any form of telephonic transmitter is suitable for use in connection with the electro-static receiver. The form employed by Prof. Dolbear is a modification of the Reis transmitter.

As we have already remarked, a high electro-motive force is necessary for the successful use of the electro-static telephone receiver. For this reason the transmitter is always used in connection with an induction coil. In order to insure the requisite electro-motive force, the secondary coil in the induction coil is made of a comparatively high resistance, such, for example, as four or five thousand ohms.

It is on account of this higher electro-motive force, that resistances inserted in the line circuit have so little effect in diminishing the efficient action of the instrument. Therefore, by the use of the electro-static telephone, articulate speech may be successfully transmitted through distances much greater than is possible with the magneto-electric telephone.

Indeed, it is not resistance that is to be avoided in the electro-static telephone but an increase in the electro-static capacity of the circuit. Now, since the electro-static capacity decreases with the decrease in the diameter of the wire, and since a high resistance is of no particular disadvantage, the finer the wire the more perfectly is the disadvantage due to the electro-static capacity of the wire obviated. If, therefore, a sufficiently fine wire could be stretched under the Atlantic, trans-Atlantic telephony would be rendered quite feasible by the use of Prof. Dolbear's inventions.

In Fig. 7, for which, in common with Figs. 3, 4, 5 and 6, the author is indebted to the *Scientific American*, of June 18, 1881, is shown the complete Dolbear telephone system.

The induction coil, *I*, is connected with the battery, *B*, as shown. The transmitter, *T*, is placed in the circuit of the battery and the primary wire of the induction coil. The receiver, *R*, is connected, as shown, to the terminals of the secondary coil of the induction coil, *I*. Similar instruments are placed at the other end of the line. When a person wishes to talk into the transmitter, a key, *b*, readily permits him to cut out his receiving instruments.

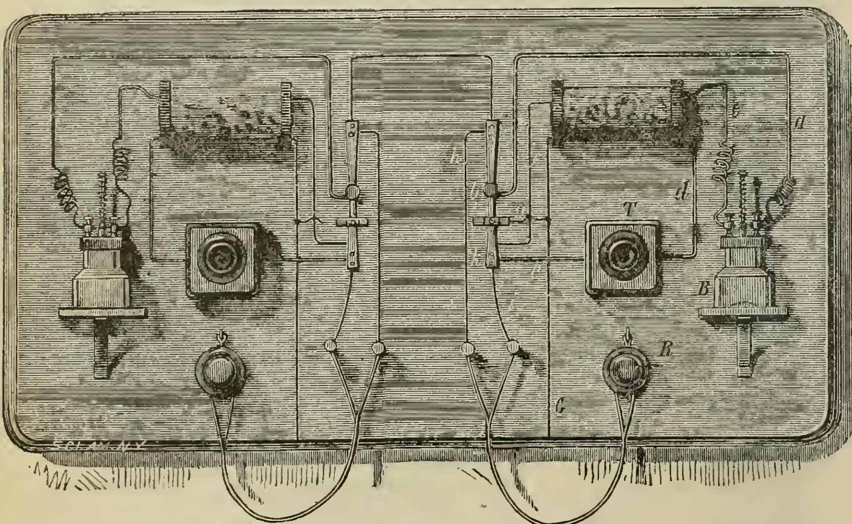

FIG. 7.—Dolbear Electro-Static Telephone System.

From the preceding description it will appear that the electro-static telephone of Dolbear forms a species of instrument quite distinct from other telephones. Although it follows Reis, in that it employs the voice of the speaker to vary the amount of battery current that traverses the primary circuit of an induction coil, yet it differs from Reis, and all who have followed him, in that it takes the electrical current so modified and utilizes it directly for the reproduction of the original speech by the attractions caused by its differences of potential. In this respect, as will be observed, it differs radically from the Bell instrument, in which the electrical current, when modified by the voice, is not directly utilized for the reproduction of the original speech, but for the purpose causing variations in the magnetism of an electromagnet, these variations in the magnetism being made to reproduce the original speech by their attractions of a magnetized diaphragm.

The electro-static telephone possesses some advantages not found in

other telephones. One of the most important of these advantages is its marked freedom from the effects of induction currents from neighboring conductors. The electro-static telephone owes this peculiarity to the high electro-motive force it necessarily employs in order to obtain the requisite movements of the plate of the receiving instrument. The electro-motive force of the currents ordinarily induced in telephones is too low to produce any noticeable effects in the electro-static instrument. There is, therefore, a marked absence of the annoying spluttering and frying sounds so common in magneto-electric telephones whose circuits extend near and parallel to other conductors. For similar reasons the electro-static telephone is much less affected by earth currents than the ordinary magneto-electric telephone.

Prof. Dolbear has invented a new form of transmitter, which he calls the battery transmitter. It consists essentially of two battery plates, separated by an electrolytic liquid. When the voice of the speaker is directed against one of the plates, it is moved thereby towards and from the other plate, and thus, decreasing and increasing the internal resistance of the battery, causes corresponding variations in the current strength produced. The battery transmitter has been found in practice to give excellent results.

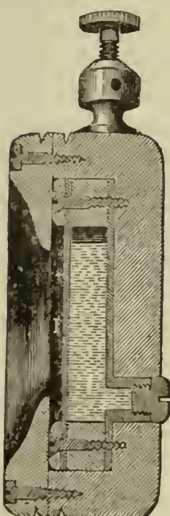

Fig. 8.—Dolbear's battery transmitter.

One form of battery transmitter is shown in Fig. 8, in which *I*, represents a plate of any suitable metal, such, for example, as zinc, while opposite it is placed a plate of carbon. These plates, when properly insulated from each other, will form a shallow cell when filled with a suitable electrolytic liquid, as shown.

If now a speaker talks into the mouth-piece, the plate *I*, is moved by the sound waves towards and from the carbon plate. The variations thus produced in the distance between the two battery plates cause corresponding variations in the strength of the current passing through the battery, in consequence of the variations in the resistance of the cells.

Central High School,
Philadelphia, November 3d, 1884.

[Note.—The cuts necessary for the illustration of the remainder of this article could not be obtained in time for the present issue of the Journal. The remainder of the article on Dolbear's researches will, therefore, appear in a subsequent number of the Journal.—Eds., J. F. I.]

GLIMPSES OF THE INTERNATIONAL ELECTRICAL EXHIBITION.

By Prof. Edwin J. Houston.

No. 3.—Gray's Telephonic Inventions.

Among the names of the distinguished scientific men who have aided in the production of the speaking or articulating electric telephone, that of Elisha Gray, of Chicago, occupies no mean position. Like most of those who aided in the invention of this marvelous instrument, the work of Mr. Gray leading towards his part of this invention dated from a time several years prior to this production of a working instrument.

The part played by Mr. Gray in the invention of the electric telephone was the natural outgrowth of his conception of his system of multiplex-harmonic telegraphy. In this system of telegraphy by means of which the multiple transmission of messages is rendered electrically possible, all the characteristics of musical sounds, the pitch, the intensity and the quality, are caused to impress their peculiarities on an electrical current, which is transmitted through a conducting wire, and is caused, by suitable electrical appliances, to reproduce, at the receiving end of the line, the peculiarities of the sounds impressed on it at the transmitting end. Mr. Gray invented this system of multiple-electric transmission early in 1874. His method consists substantially in sending into the line, by the vibration of tuning-forks, or tuned reeds, separate series of electrical impulses, that vary both in their strength and in their rapidity. There is thus charged on the line electrical impulses varying considerably both in their rapidity and in their strength.

At the distant end of the line a series of electro-magnets are placed, the armatures of which are rigidly attached at one of their ends to one of the poles, the other end being free to vibrate towards and from the free pole. If then the end of the line be connected to a number of different electro-magnetic receivers, the rate of vibration of which exactly corresponds with the rate at which the separate series of electrical impulses were sent into the line, each of the receivers will be actuated by those electrical impulses, that correspond in frequency to the frequency with which it swings, and by those impulses alone.

A number of independent messages, therefore, can thus be sent over the line without in any respect interfering with one another.

Mr. Gray carried on an extensive series of experiments with his multiple-telegraph apparatus, the natural outgrowth of which resulted in his filing in the United States Patent Office, on the 14th of February, 1876, a caveat for "a new art of transmitting vocal sounds telegraphically." In this caveat he specifically states, "It is the object of my invention to transmit the tones of the human voice through a telegraphic circuit, and reproduce them at the receiving end of the line, so that actual conversation can be carried on by persons long distances apart."

Singularly enough, this specification was filed in the United States Patent Office on the same day that Alexander Graham Bell filed his specification for "a method of, and apparatus for, transmitting two or

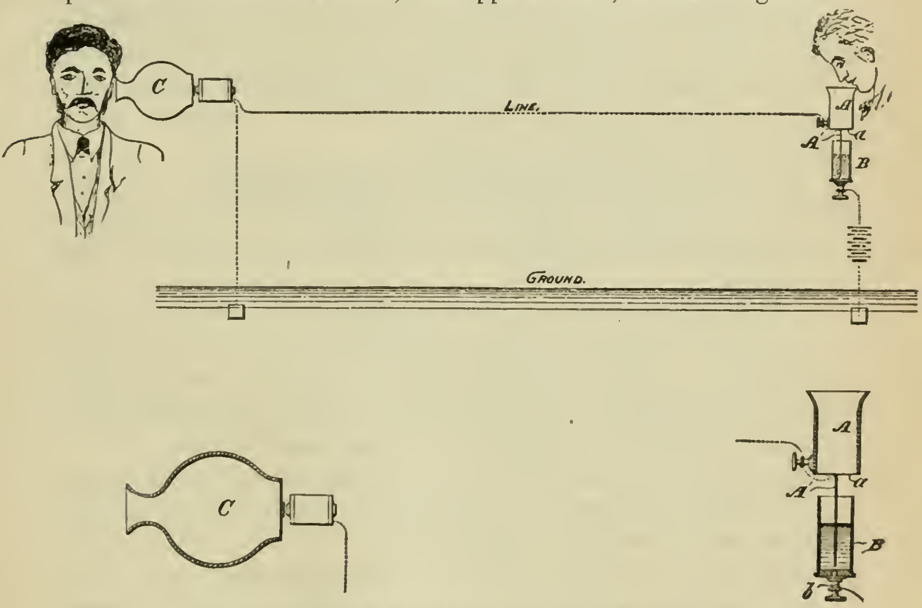

FIG. 1.—Gray's first articulating telephone (caveat design).

more telegraphic signals simultaneously along a single wire by the employment of transmitting instruments, each of which occasions a succession of electrical impulses differing in rate from the others; and of receiving instruments, each tuned to a pitch at which it will be put into vibration to produce its fundamental note by one only of the

transmitting instruments." This is the specification on which Bell rests his claims for the invention of his first articulating telephone.

Thus it will be seen that by a remarkable coincidence these two applications for government protection for the invention of telephones were not only filed on the same day, but were also the direct outgrowth of two very similar systems of multiple electric telegraphy.

In Fig. 1, is shown the instrument as figured in Mr. Gray's caveat of February 14, 1876.

In this early form of instrument the transmitter is shown at *A*. The instrument, therefore, belongs to that class of telephones in which the voice of the speaker is caused to vary an electrical current by producing variations in the resistance of its circuit.

In Fig. 1, a battery, grounded at one end as shown, has its other terminal connected to the vessel *B*, "filled with some liquid possessing high resistance, such, for instance, as water, so that the vibrations of the plunger or rod A^1, which does not quite touch the conductor *b*, will cause variations in the resistance, and, consequently, in the potential of the current passing through the rod A^1." The part quoted is taken from the language of the caveat.

The mouth-piece, *A*, has attached to it a diaphragm, *a*, "of some thin substance, such as parchment or gold-beaters' skin, capable of responding to all the vibrations of the human voice, whether simple or complex."

A receiving instrument is shown at the other end of the line, at *C*, and consists of "an electro-magnet of ordinary construction, acting upon a diaphragm to which is attached a piece of soft iron, and which diaphragm is stretched across a receiving vocalizing chamber, *C*, somewhat similar to the corresponding vocalizing chamber *A*."

"The diaphragm at the receiving end of the line is thus thrown into vibrations corresponding with those at the transmitting end, and audible sounds or words are produced."

"The obvious practical application of my improvement will be to enable persons at a distance to converse with each other through a telegraphic circuit, just as they now do in each other's presence, or through a speaking tube.

"I claim as my invention the art of transmitting vocal sounds or conversations telegraphically through an electric circuit."

The invention thus embodied by Gray in his caveat of 1876, he claims, was conceived by him at a much earlier date, but owing to press of other electrical work, was not embodied in an actual written

description until shortly before the time of filing the caveat. The idea of the invention was suggested to him by the string telephone, and an examination of Fig. 1, will show that he has, to a certain extent, copied this instrument electrically.

Unfortunately for Gray, he did not have a working model of his telephone completed until after Bell had modified the instrument described in his application for letters-patent filed on the same day as that on which Gray deposited his caveat in the Patent Office. We say unfortunately, since Gray's first instrument would transmit intelligible, articulate speech, while Bell's original instrument would not.

In order the better to compare the invention set forth in the caveat of Gray with that of Bell, we will annex a short description of that part of the apparatus described in Bell's application of February 14, 1876, the application that is claimed to cover the invention of a telephone for the electrical transmission of articulate speech. This form of instrument is shown in connection with Fig. 2, which is taken directly from figure 7 of Bell's specification.

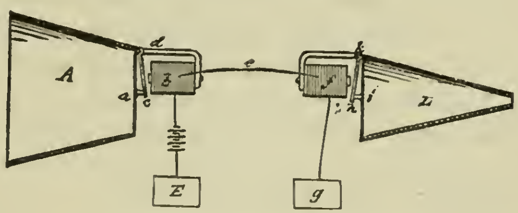

Fig. 2.—Bell's original instrument.

This apparatus is described in his specification as follows, viz.:

"The armature *c*, is fastened loosely by one extremity to the uncovered leg *d*, of the electro-magnet *b*, and its other extremity is attached to the centre of a stretched membrane, *a*. A cone, *A*, is used to converge sound-vibrations upon the membrane. When a sound is uttered in the cone the membrane *a*, is set in vibration, the armature *c*, is forced to partake of the motion, and thus electrical undulations are created upon the circuit *E*, *b*, *e*, *f*, *g*. These undulations are similar in form to the air vibrations caused by the sound; that is, they are represented graphically by similar curves. The undulatory current passing through the electro-magnet *f*, influences its armature *h*, to copy the motion of the armature *c*. A similar sound to that uttered into *A*, is then heard to proceed from *I*."

This instrument does not appear to have been capable of transmit-

ting intelligible, articulate speech, in the form in which it is figured and described in Bell's early specification. The best results he was able to obtain from it were faint sounds, heard by his assistant, but not verified by Bell. Or, quoting the language employed by Bell in a lecture delivered before the Society of Telegraph Engineers, October 31, 1877: "The results were unsatisfactory and discouraging. My friend, Mr. Thomas A. Watson, who assisted me in this first experiment, declared that he heard a faint sound proceed from the telephone at his end of the circuit, but I was unable to verify his assertion. After many experiments attended by the same only partially successful results, I determined to reduce the size and weight of the spring as much as possible. For this purpose I glued a piece of clock spring, about the size and shape of my thumb nail, firmly to the centre of the diaphragm, and had a similar instrument at the other end; we were then enabled to obtain distinctly audible effects."

Before Gray had a working model made in accordance with his caveat, Bell modified his apparatus in the manner above described, and had successfully transmitted intelligible, articulate speech. This was unfortunate for Mr. Gray, since his apparatus, when constructed as described in his caveat, acted successfully as a speaking telephone. Bell, having first reduced his invention to practice, was successful in establishing his rights over those of Gray. From the record, however, Gray is clearly entitled to at least share with Bell the honor for the invention of this form of articulating telephone.

The modified form of Bell receiver, by means of which he obtained his first satisfactory, intelligible, articulate speech, is similar to that shown in (section) Fig. 3.

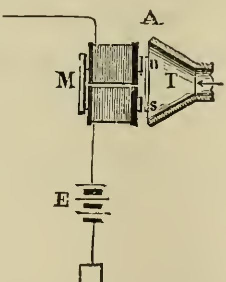

FIG. 3.—Bell's modified telephone receiver (section).

This receiver consists, as shown, of an electro-magnet *M*, opposite the poles of which is a tightly-stretched membrane which closes a

mouth-piece *T*. A piece of watch spring in the form of a permanent magnet, *n, s,* is rigidly attached to the middle of the membrane. This modified form of receiver is shown in elevation in Fig. 4.

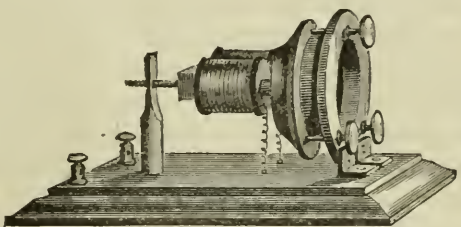

FIG. 4.—Bell's modified receiver (elevation).

Continuing his experiments as to the most favorable conditions for the proper working of his telephone, Bell made numerous experiments as to the influence produced by variations in the size of the different parts. Among other things, he gradually increased the size of the magnet fastened to the diaphragm of gold-beater's skin, until at last he he entirely replaced the gold-beater's skin diaphragm by one of iron, and thus vastly improved the efficiency of his instrument.

While experimenting on the simultaneous multiple transmission of various tones electrically, Gray made several forms of receiving instruments for causing such tones to be heard at the distant end of the line. One of these, which was devised some time before the form shown in the caveat of February 14, 1876, was suggested by a sound heard from a bath tub, to the metallic lining of which one of the terminals of the secondary wire was attached. This sound was heard when the dry hand, grasping the other terminal, was slid over the lining. Gray experimented at considerable length on this curious observation, and soon found that a receiver of this character was able to reproduce sounds of varying pitch, so that a tune played at the distant end of the line was distinctly reproduced by the receiver.

In order to readily obtain the continuous sliding motion necessary in this form of receiver, Mr. Gray mounted a thin, cylindrical, resonant box of wood on a horizontal axis; on the cylindrical surface of the box was placed a slightly convex, metal cap. The metal cap was connected to the ground by means of a wire. When the end of the line was held in the hand and the fingers were pressed against the metal cap, musical sounds made at the distant end of the line were distinctly heard when the cylinder was revolved. When the current passing is sufficiently great,

these sounds are readily heard at some distance from the receiver. Within certain limits they increase in loudness when the crank is turned more rapidly.

Gray did not appear at first to understand the manner in which these receivers operated. Later, however, he attributed them to the slipping of the hand on the metallic surface due to the variations in friction produced by the current. He noticed an increase in the friction whenever the key was closed. This principle, it will be observed, is quite similar to the principle employed by Mr. Edison in his motograph.

Gray patented this form of receiver in connection with a telephonic

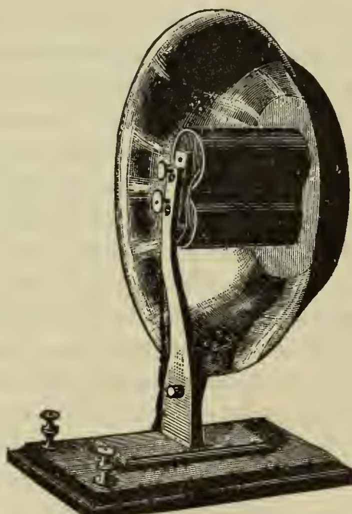

FIG. 5—Gray's Telephone Receiver.

transmitter that was able to transmit articulate speech, in 1878. He obtained the necessary electro-motive force of the electrical currents requisite for the successful operation of the receiver by the use of an induction coil. He points out in this patent the fact that other animal tissues may be employed in the place of the fingers of the hand. In this application the receiver is figured as a highly polished metallic disc of thin sheet metal, mounted on a resonant box or case, provided with a shaft so as to be readily revolved.

An exceedingly simple, yet quite efficient receiver is shown in Fig. 5. In this form, an iron vessel, supported as shown, is firmly fixed in

an upright position by one of its edges. An electro-magnet, separately mounted on the same base, has its poles near, but not in contact with the bottom of the pan. Under these circumstances the bottom of the pan acts as a magnetic diaphragm, and emits musical or other sounds, uttered at the other end of the line, with great distinctness.

The resemblance of this form of receiver with that now employed in the articulating telephone is too evident to need comment. It will speak quite plainly when placed in connection with any form of articulating transmitter, such for example as that figured in the Gray caveat, or with a Reis transmitter. Indeed two such instruments placed at the extremities of a circuit in which is included a battery, form a complete telephonic system, since one of them can be used as a transmitter, while the other will act as a receiver.

Although these receiving instruments were not used by Gray to receive articulate speech prior to the actual use by Bell of his form of articulating telephone, yet they are of interest when taken in connection with the actual position that Gray occupied as regards the articulating telephone as exhibited by the description contained in his caveat of February 14, 1876. In that paper he clearly expresses the objects of his invention as being means for varying an electric current by the human voice for the purpose of throwing on a telegraph line such variations as will, when suitably received at the distant end of the line, reproduce the voice. He figures a form of transmitting instrument that will so permit the voice to vary the current of a voltaic battery and a form of receiver that will act in connection therewith. He is operating with a current that is greatly varied, and contrives for its reception a receiver that is capable of responding to a wide range of variations. Since then the receiver shown in Fig. 5, was devised for exactly the same purpose, he is, it would seem, be entitled to use it to replace the receiver shown in his caveat already referred to.

On the 29th of October, 1877, Gray filed a number of applications for letters patent, viz.: one for the "art of transmitting vocal sounds telegraphically," one for "certain new and useful improvements in apparatus for transmitting vocal sounds telegraphically," and one for "certain new and useful improvements in electric telephony." In the latter application he figures substantially the telephone receiver, which is shown in connection with Fig. 5. This he styles his "concave diaphragm receiver," for speaking telephones. His application shows

similar instruments at each end of the line, for receiving and transmitting instruments respectively.

Gray has added a number of important features to the articulating telephone as it is to-day commercially employed. He took out a number of patents in 1878. Among other instruments he devised a method by means of which the weakening effects on the electric currents transmitting articulate speech, due to their passage through a number of instruments, calls, etc., placed in series in the main line, were decreased. This method consisted essentially in the use of condensers.

In the same year he invented a contrivance by means of which all sounds not transmitted through the line wire are excluded from the observer's ear. This he effected by mounting two telephones on a common support so that they could readily be simultaneously applied to both ears of the person receiving the message. The two telephones were mounted on rocking bearings so as readily to adapt themselves to the ear of the listener. At the same time the handle of the common support, on which they were mounted, was made so as to readily be grasped in one hand, thus leaving the other hand free to hold an ordinary transmitting instrument to the mouth, when it was desired to send a message.

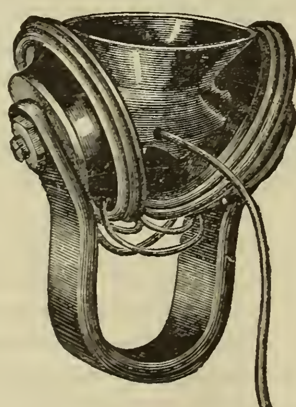

Fig. 6.—Gray's Bi-polar Telephone.

Gray invented his bi-polar telephone, shown in Fig. 6, in 1878. In this instrument, as the name indicates, there are two magnet poles, each of which is caused to act on a separate metallic diaphragm, inclined at such an angle with respect to each other as to permit the use of a single

mouth-piece placed between them. A permanent magnet, U-shaped, serving as the handle of the instrument, has two electro-magnets secured to its two poles. The soft iron cores of these magnets are provided with the usual coils of insulated wire. The cores are attached to the permanent magnet by securing them into ends thereof, so that they are thus readily adjusted with respect to the iron diaphragms, of the usual construction placed, opposite them.

In the same year Gray extended the idea contained in his bi-polar telephone and constructed a device which he termed his "duplex-bi-polar telephone." The object of this instrument is to increase the volume and clearness of the sound. This he endeavored to secure by the combination with a common mouth-piece, of a series of diaphragms with their corresponding magnets "arranged in pairs on the outer, opposite sides of the poles of a permanent magnet and in a reverse order relatively thereto, in such a manner that the secondary diaphragms, and the magnet of each pole will be actuated directly from or by its primary diaphragm."

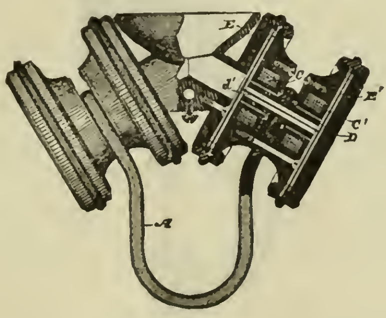

FIG. 7.—Gray's Duplex-Bi-polar Telephone.

Gray has produced a number of modifications of his bi-polar instrument. Besides the duplex bi-polar telephone above described, he patented, in 1878, another modification intended to increase the electro-motive force of the generated current, and to obtain an increased volume and clearness of the sound produced. In one form he shows a bi-polar telephone "with its diaphragms and electro-magnets duplicated, and arranged in reverse order on each side of the poles of its permanent magnet. The soft iron core of the electro-magnet is made tubular. Through this core a light rod of wood or other non-magnetic material passes. The ends of this rod are rigidly attached to the centres of both diaphragms, so that the move-

ment of either of the diaphragms produces corresponding movements in the other diaphragm. By this arrangement the inventor is enabled to vibrate two diaphragms before two electro-magnets, both of which receive their magnetic charge from the same pole of the permanent magnet. The other pole of the permanent magnet has similar apparatus connected with it, so that the inventor thus obtains the current produced by the vibrations of four separate diaphragms.

This form of telephone is shown in Fig. 7. The U-shaped steel magnet is seen at A. The tubular soft iron core, D, is provided with helices C, C^1, and diaphragms E, E^1. The rod d^1, intended to carry the motion of the diaphragm nearest the mouth to the other diaphragm, is connected to the two diaphragms as shown. On the left hand side of the drawing similar parts are shown in elevation.

In Fig. 8 is shown Gray's electro-magnetic telephone. This instru-

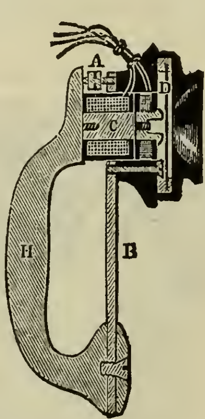

FIG. 8.—Gray's Electro-magnetic Telephone (section).

ment consists of a handle, H, of iron or steel, furnished with a soft iron core, C, conneeted by means of a screw, with one end of H in the manner shown. This core is wrapped with a coil of wire of comparatively low resistance that is included in the circuit of a voltaic battery of two or three cells. Opposite to the core of this magnet, and mounted thereon in the same plane, but separated therefrom by some non-magnetic material, is placed a second electro-magnet. The core of this magnet is made of a slotted tube of soft iron. The coil of wire wrapped thereon is made of a much higher resistance than the coil in the first magnet. A stiff spring-plate, B, of iron or steel is

secured at one end to the handle, H, and at the other end is perforated so as to permit the magnet core, C, and its coil to pass through it. This metallic plate is securely attached at its upper edge to the edges of the magnetic diaphragm. In this manner, as will be seen, the

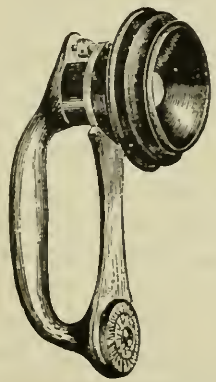

FIG. 9.—Gray's Electro-magnetic Telephone (elevation).

opposite poles of the magnet are brought into close proximity with each other, and their magnetic effects greatly increased.

The idea of thus connecting the diaphragm with the opposite pole of the magnet has been employed in a number of different telephones.

In Fig. 9 is shown the elevation of this form of speaking telephone.

CENTRAL HIGH SCHOOL,
Philadelphia, Nov. 15, 1884.

ABNORMAL FLOWERS.—A young botanist found, on the borders of a forest, near Brussels, a poppy in which the four petals, by the union of their opposite edges, had become tubular, each presenting the appearance of a monopetalous infundibuliform corolla, giving the flower a strange but somewhat elegant appearance. Transformations of leaves into tubular or vermiform organs are normal in some plants, such as the nepenthe, the sarracenia, etc., and accidental transformations are not uncommon. In the vernation of the poppy each petal is folded upon itself, in such a way that its lateral borders coincide throughout their whole length. It is not then so astonishing that they should sometimes unite, as that they unite so rarely. There is one cultivated variety of poppy, the *papaver bracteatum*, in which the petals are joined at their borders, so that the flower presents the appearance of a large cup.—*La Nature*, May 24, 1884. C.

THE EARTH'S ELLIPTICITY.

By L. d'Auria.

In the October number of the Journal of the Franklin Institute, Prof. Chase states that in my paper of August (this Journal) I have introduced some of the elements omitted in my former paper on the Ellipticity of Planets, and takes the credit to have called my attention to it by his criticism. This statement is incorrect, for I have treated the same general equation with the same elements as before in the paper quoted, only have amended an error which occurred in assigning the limits to certain terms contained in such equation.

Moreover, the professor declares untenable the hypothesis which I was forced to admit, viz., that the earth's ellipticity may have been formed and retained at a time when gravity at the poles was almost equal to gravity at the equator, and that from such time gravity at the poles has been increased over that at the equator by some unknown cause, without, however, being able to affect the already formed ellipticity, on account of the earth's rigidity.

The only part of this hypothesis which may seem hard to admit is how, without any change of form in the earth, the ratio of gravity at the poles to that at the equator could undergo any variation whatever. In answer to this I would respectfully say that it is very doubtful whether any scientific man is prepared to tell us what variation the specific gravity of bodies at the earth's surface would undergo were the earth deprived of its magnetic properties. Could it not be that the increase of the above ratio from 1 to 1·00167, as required by my analysis, is due to magnetism?

The professor will be surprised, perhaps, to learn that in the "Phil. Trans." for 1847 there is a memoir, by Hearn, entitled: "On the cause of the discrepancies observed by Mr. Baily with the Cavendish apparatus for determining the mean density of the Earth," where these discrepancies are attributed to the influence of magnetism.

Prof. Chase tests my analysis by his harmonic theory of the universe. According to this theory, he insists that the ratio

$$(v_2 \div v_1)^2,$$

must be the measure of the earth's oblateness, and seems to be unaware of the fact that such ratio is simply that of centrifugal force to attrac-

tion at the equator. In fact, $v_1 = \sqrt{gr}$, where r is the equatorial radius, g gravity at the equator, and v_2 is the actual velocity of rotation of the earth at the equator. Hence

$$(v_2 \div v_1)^2 = \frac{v_2^{\ 2}}{gr} = \frac{v_2^{\ 2}}{r} \div g;$$

and since $\frac{v_2^{\ 2}}{r}$ represents centrifugal force at the equator, it follows that the professor, without knowing it, insists that the oblateness and the ratio of centrifugal force to attraction at the equator are one and the same thing.

By the same harmonic theory, Prof. Chase, in this JOURNAL for May, 1884, computes the sun's distance, and substantially puts it as follows:

$$\text{Sun's dist.} = \tfrac{5}{2} \times \tfrac{3}{2} \times 31{,}558{,}149 \times \sqrt{gr} \div 2\pi;$$

where 31,558,149 is the period of revolution of the earth around the sun expressed in seconds. Since g is directly proportional to r, we can put

$$g = r \times \text{constant} = Cr;$$

and consequently it follows that

$$\text{Sun's dist.} = r \times \text{constant};$$

for the fractions $\frac{5}{2}$ and $\frac{3}{2}$ remain the same independently of r.

If we suppose the earth to be condensing into a smaller volume; it should be expected that by such process we would be brought nearer to the sun in proportion as the radius of the earth diminishes. Is Prof. Chase aware that such absurdities are sheltered by his new science?

WEIGHT OF DROPS.—Boymond has lately published an interesting notice upon the weight of drops. It is well known that the weight depends upon the exterior diameter of the tube; the interior diameter having no influence except upon the velocity of flow, the nature of the liquid determines the weight, whatever may be the proportion of dissolved material that it contains. Boymond used a dropper of three millimetres diameter and determined the weights by an extremely sensitive balance. The mean of his results gave, in a gramme of distilled water, 20 drops; alcohol of 90°, 61 drops; alcohol of 60°, 52 drops; alcoholic tinctures from 60° to 90°, 53 to 61 drops; etherial tincture, 82 drops; a fatty oil, about 48 drops; a volatile oil, about 50 drops; an aqueous solution, whether diluted or saturated, 20 drops; a medicinal wine, 33 to 35 drops; laudanum, about 33 to 35 drops.—*La Nature*, April 12, 1884. C.

STANDARD SIZES FOR HEXAGON BOLT HEADS AND NUTS.

[In *Mechanics*, November, 1884, a communication on the above caption from Mr. C. E. Simonds, East Cambridge, Mass., was published. This we reproduce with some remarks by Mr. Coleman Sellers, Professor of Mechanics in the Institute, who was a member of the Committee of the Franklin Institute that reported on this subject December 15th, 1864.]

"The advantages of a system by which certain classes of machine-work can be constructed and placed upon the market are of untold value so long as the product is constructed according to the rule given. Adopting a rule is one thing, but universally working to it is another matter. On December 15, 1864, a system of bolt heads and nuts, as well as screw threads, was recommended and adopted by a committee of the Franklin Institute of Philadelphia, and adopted by the United States Government in May, 1868. It is well, perhaps, to state that this system is known as the Sellers' or Franklin Institute, likewise the United States' standard. The sizes or dimensions of the width of the parallel sizes of a hexagon bolt head and nut are $1\frac{1}{2}$ times the diameter of the bolt plus $\frac{1}{8}$ inch when the nut or bolt head are in the rough state; thus it will be seen by the rule that a nut or bolt head for a 1-inch bolt should measure $1\frac{5}{8}$ inches. For a finished hexagon bolt head and nut the rule is $1\frac{1}{2}$ times the diameter of the bolt plus $\frac{1}{16}$ inch, and the nut and bolt head should thus measure $1\frac{9}{16}$ inches.

"The writer, some time ago, furnished drawings for a complete set of wrenches of the drop-forged system, and the sizes given were for bolts from $\frac{1}{4}$ inch to 6 inches inclusive. A large number of the wrenches were milled exact to size, case-hardened and placed upon the market, and the undertaking brought to light the astonishing fact that of all the manufacturers of hexagon bolt heads and nuts in the United States not one can be found who manufacture nuts and bolt heads according to the rule as given by the Franklin Institute. Messrs. Hoopes & Townsend, of Philadelphia, in their catalogue state that the nuts on pages 20 and 21 are United States' standard sizes and have been adopted as standard by several of the most prominent railroads. Pages 20 and 21 gives sizes of bolts and nuts from $\frac{1}{4}$ inch to 3 inches, the list including cold-punched, chamfered and trimmed nuts; also cold-punched nuts for cars and unfinished work. It is to be borne in mind that the manufacturers state that they are United States' standard

sizes. If the rule of $1\frac{1}{2}$ times the diameter of the bolt plus $\frac{1}{16}$ inch be correct, how is it that Messrs. Hoopes & Townsend, as well as all of the manufacturers of nuts and bolts, make their product $\frac{1}{16}$ inch larger, and distinctly state that the thread and outside of each bolt head and nut are made to an accurate gauge and to the standard adopted by the United States Government? What are mechanics to understand by these statements? Do goods manufactured $\frac{1}{16}$ inch over size mean that they are to be classed with articles manufactured $\frac{1}{16}$ inch less in size, which is the true size.

"Page 42 of their catalogue gives proportions for United States standard screw threads and nuts. Taking from their table a bolt of 1-inch diameter, it is shown that the width of the parallel sides of a nut are $1\frac{5}{8}$ inches; according to the Sellers or Franklin Institute system it should be $1\frac{9}{16}$ inches. Take, again, the list of sizes as given on page 7, which, by the way, are stated to be standard sizes of heads for bolts—it will be noticed that the word standard is affixed to everything on this page—$1\frac{9}{16}$ inches is given for the size of nut for a 1-inch bolt. This is correct. What does this mean? Perhaps Messrs. Hoopes & Townsend will explain what the word standard means. Haswell's pocket edition for mechanics and engineers for 1870 gives on page 123 a list of sizes of nuts and bolts from $\frac{1}{4}$ inch to 6 inches inclusive. This list gives as a rule $1\frac{1}{2}$ times the diameter of the bolt for the width of the nut, and hence by this rule we have another size of nut. On the next page Mr. Haswell gives the sizes of screw threads, bolt heads and nuts as per rule of the Franklin Institute. Why Mr. Haswell gives two sizes of nuts for the same bolt is a mystery to the writer. The sizes thus shown in this edition are $1\frac{1}{2}$ and $1\frac{9}{16}$ inches for a bolt of 1 inch diameter. In the revised edition for 1884, page 157 gives $1\frac{1}{2}$ times the diameter of the bolt for the width of the head, and $1\frac{1}{2}$ times the diameter plus $\frac{1}{8}$ inch as the width of the parallel sides of a hexagon nut. Rather poor information to be found in a revised edition. Mr. Trautwine in the revised edition of his pocket-book gives as a rule $1\frac{1}{2}$ times the diameter of the bolt for the width of the parallel sides of a hexagon bolt head and nut, and he says that some machinists add $\frac{1}{8}$ inch to this for all diameters of bolts. The word *some* is very amusing, taken as it is from the pages of a work devoted to the interests of mechanics.

"In *Mechanics* for March 18, 1882, a correspondent asks for information regarding the sizes of standard bolt heads and nuts of the Frank-

lin Institute system, in which he feels sure that it would be conferring a great favor upon many readers, and would at the same time enlighten many workmen who are not posted upon the subject. The sizes, as well as the information thus given, are taken from the revised edition of Mr. Nystrom's pocket-book, and it is stated that the sizes are those of the Franklin Institute system, which, by the way, is not the case, inasmuch as the sizes given are $\frac{1}{16}$ inch over size. At the end of the article it is stated that the workman should bear in mind that these dimensions are finished sizes, and are intended for use on either black or bright work; and, furthermore, that the same size wrench will fit either finished or unfinished bolt heads and nuts. The *Scientific American Supplement*, No. 443, page 7072, for June 28, 1884, contains the best and only correct table ever published. To the railroad master-mechanic, master car-builders and mechanics in general the questions arise, Is it possible to ever correct the error which has existed for a period of over 20 years? Is it not best to drop the word standard, and forget that there was ever such a word? It looks something like counting chickens before they are hatched. We have got the word standard, but where are we to find the nuts or the manufacturers who know what the standard is and who work to it?"—*C. E. Simonds, in Mechanics.*

REMARKS ON THE FOREGOING:

The Franklin Institute standard of *bolt threads* is the same as that of the United States Government, as fixed by the board under Mr. Isherwood, then Chief of the Bureau of Steam Engineering, May 15, 1868. In regard to the size of the heads of bolts and also the size of nuts the Franklin Institute standard *is not* the same as that adopted by the United States Board and now called the United States standard. Mr. Simonds correctly states the Institute standard for finished bolt heads and nuts to measure, for the width between the parallel sides of hexagon nuts, once and one-half the diameter of the bolt plus one-sixteenth of an inch, That is to say, rough bolts made one and one-half plus *one-eighth* of an inch can be planed or dressed to the finished size, there being sufficient stock for that purpose. The Government Board decided that all bolt heads and nuts should be made the same size, whether black (that is rough) or finished, so that the same wrench could be used on either. Unfortunately they adopted as the size of the finished bolt head and nut the rough size, as indicated by the

Franklin Institute committee. Messrs. Hoopes & Townsend are hence quite correct in calling their product the United States standard. Accepting the fact that there has thus come to be a discrepancy between the standard recommended by the Institute and the United States, it may be well to say a few words about the principle involved. I may say that there are many very large concerns, which, having adopted the Franklin Institute standard of bolt heads and nuts, have come, since the introduction of the improved methods of making forgings and cold pressed nuts, to call all nuts and bolt heads *finished* that are made to gauge either in the smith shop or in the machine shop. It would be doing injustice to the very good nuts, for instance, made by Messrs Hoopes & Townsend, to call their cold pressed nuts, sized by passing through gauged dies, "unfinished" or "rough" nuts. They are as accurate to size as the nuts finished and polished in many machine shops. It is no very easy matter, nor is it wise, for those who have adopted the smaller size of the Franklin Institute, to revert to the larger size recommended by the Government Board. Machines having bosses raised on the patterns for the bolt heads and nuts, will require such bosses to be enlarged for the larger size. Bolts placed near to flanges, in corners or near edges, will have to be moved away from the flange or farther from the corner to allow room for the use of wrench. Good engineering calls for the head and the nut to be as small as is consistent with strength and utility in manipulation, to permit the placing of the bolt in the best position, that is, in the closest proximity to flanges and in corners. Very often the removal to a greater distance from the edge will necessitate the use of a larger bolt. All these matters were considered by the committee of the Institute, and confirm the writer in his preference for the standard of the Franklin Institute, as all bolts can be reduced in size of head without prejudicing their position, while enlargement involves practical difficulties without any good result. We have now two standards, and, that there are many who know how to work to either, is shown by the fact that all makers of bolts and nuts furnish what they are required to do under one or the other name, even if they do only publish as standard that adopted by the Government Board.

COLEMAN SELLERS,
Professor Mechanics, Franklin Institute.

PHILADELPHIA, November 14, 1884.

Book Notices.

THE CAR-BUILDERS' DICTIONARY: an Illustrated Vocabulary of Terms which designate American Railroad Cars, their Parts and Attachments. Compiled for the Master Car-Builders' Association, by Matthias N. Forney, Mech. Eng., assisted by Leander Garey, Superintendent of the Car Department, N. Y. C. and H. R. RR., and Calvin A. Smith, Secretary Master Car-Builders' Association (3d thousand). New York: Published by the Railroad Gazette, No. 73 Broadway, 1881.

This publication is a very valuable contribution to information of practical men, and is of a class much needed; its scope of usefulness is really much greater than its title would imply; the illustrations are far more complete and exact than are usually to be found in technical lexicons, proportions and dimensions of parts being indicated in most of its illustrations as fully as in most large cyclopedias and with a degree of exactness indicating much conscientious painstaking, the devices are those in actual practice and use rather than the contrivances which expand the records of the Patent Office. In the present state of railway practice and the necessity for cheapening charges for transportation, everything that contributes to the facility of making cars interchangeably useful on all railways is of great inportance, and to facilitate this result, the effort of this book is to secure exact and uniform understanding of terms employed throughout the country. This is not only in the proper direction, but is as well carried out as the nature of its subject-matter will permit. The modest apology for the appearance of advertisements in the work was hardly necessary, they are so well indexed that they really contribute valuable information to those in the craft, and whilst not perhaps just to the taste of many library readers, their presence in the work will, if we mistake not, be considered desirable and important by those practically engaged in railway operations and the manufacture of railway supplies.

Books of this scope and character are undoubtedly a great need of these times, and this appears to be both happily conceived and well executed.

Few books afford as much exactly stated information in so concise and easily accessible form, and the endorsement of the Master Car-Builders' Association makes it an authority for the craft. S. L. W.

CORRESPONDENCE.

EARLY COMPOUND ENGINES, AN ITEM OF HISTORY.

Committee on Publication of the Journal of the Franklin Institute:

GENTLEMEN:—In an article on the trial of the Steamer "City of Fall River," in the July number of the JOURNAL, allusion is made to early compound engines in this country, by note, as follows: "The first compound engines are, however, said to have been built by an engineer of a still earlier generation, Mr. I. P. Allaire." As early as 1830 or 1832 there were on the Hudson River two steamboats with compound engines, the

Swiftsure and *Commerce*. Their engines were of the upright square form, or cross-head pattern (very few of that form now in use and none built). The high-pressure cylinder being forward and the low-pressure being abaft the paddle-wheel shaft, and both connected to it by cog-wheel gearing. About the same time the *Post Boy*, with similar machinery, built by Mr. Allaire, was sent to New Orleans. In the machinery of the above steamers the exhaust steam of the high-pressure cylinder passed directly to the low-pressure cylinder without the intervention of valves or receiver between the two cylinders. The *Swiftsure* and *Commerce* were in use for several years, and the machinery of the former subsequently taken out and replaced by the ordinary beam engine. The compound engine, built by the late Erastus Smith was of the ordinary beam pattern, except that it had two steam cylinders, the high-pressure being *within* the low-pressure one. Their diameters were thirty-seven and eighty inches, and stroke of piston eleven feet. This form has not been duplicated. The present compound engine has practically but little resemblance to those that preceded it, and is very much more economical. B. H. B.

PHILADELPHIA, October, 1884.

Franklin Institute.

[*Proceedings of the Stated Meeting, held Wednesday, November* 19, 1884.]

HALL OF THE INSTITUTE, November 19, 1884.

The President, Mr. William P. Tatham, in the Chair.

Present, 115 members, and 20 visitors.

The Actuary presented the following resolution, passed at the stated meeting of the Board of Managers, held Wednesday, November 12th, viz.:

Resolved, That the Board of Managers recommend to the Institute the necessity ; in view of the rapidly increasing value and extent of its library, the absence of proper accommodations for the same, and for other branches of the Institute's work, that active efforts be at once commenced to obtain subscriptions for the erection of a more commodious and fire-proof building.

Seventy-four (74) persons were elected to membership in the Institute since the last meeting.

The Secretary, by direction of the Committee on Science and the Arts, reported that the Committee had recommended the award of the "John Scott Legacy Premium and Medal" to G. Morgan Eldridge, of Philadelphia, for his "Improvement in Electro-Magnetic Protectors for Electrical Instruments," that the recommendation had been advertised for three months, as prescribed in the regulations, and that no objection thereto had been received.

On motion of Mr. H. R. Heyl, seconded by Mr. Hector Orr, it was

Resolved, That the recommendation of the Committee on Science and

the Arts to award the "John Scott Legacy Premium and Medal" to G. Morgan Eldridge, of Philadelphia, for his improvements in "Electro-Magnetic Protectors for Electrical Instruments" be approved; and that the Secretary be directed to notify the "Committee on Minor Trusts of the Board of City Trusts," of the action of the Institute.

Mr. Coleman Sellers presented a report on the "Tercentenary of the University of Edinburgh," to which he was appointed to represent the Institute. Mr. Sellers also made, by request, some comparative "Remarks and Comments on the Condition of the Mechanic Arts Abroad and at Home, and on Technical Education."

On Mr. Orr's motion, seconded by Mr. Washington Jones, the meeting passed a vote of thanks to the speaker for his interesting and instructive discourse.

Mr. Seller's remarks have been referred to the Committee on Publication.

The Secretary's report embraced a detailed description of the experiment of purifying the water pumped into certain of the reservoirs in the city of Philadelphia, by aeration, on the large scale. These experiments were conducted by Col. William Ludlow, Chief of the Water Department, at the suggestion of Professor Albert R. Leeds, Chemist to the Department.

The proposed amendment to Article III, Section I, of the By-Laws, offered by Mr. J. D. Rice at the October meeting, and postponed to the present meeting, was called up for consideration as deferred business. Mr. Rice advocated his proposition at length, when, on motion of Mr. Coleman Sellers, seconded by Mr. G. Morgan Eldridge, the subject was laid on the table.

The consideration of the resolution previously reported from the Board of Managers then came up, and, on Mr. Coleman Sellers' motion, seconded by Dr. Ziegler, it was

Resolved, That it is the sense of this meeting that a committee be appointed to secure subscriptions to a fund for a new building.

The President appointed to serve on this committee, Messrs. Charles H. Banes (Chairman), Chas. Bullock, Fred'k Graff, Wm. D. Marks and Coleman Sellers.

Mr. Eldridge advocated the holding of an exhibition as the best means of accomplishing the object contemplated in the above resolution. He therefore offered the following:

Resolved, That it is the sense of the Franklin Institute that an exhibition shall be held in the Exhibition Building, in West Philadelphia, during the autumn of 1885, and that the Board of Managers be requested presently to instruct the Committee on Exhibitions to take the steps necessary for the holding of the said exhibition.

The resolution was seconded by Mr. Coleman Sellers, and, after some discussion as to the propriety of devoting the contemplated enterprise to a Health Exhibition, upon the general plan of those held in Berlin, and, more recently, in London, was carried unanimously.

Adjourned.

WILLIAM H. WAHL, *Secretary.*